MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH OTHER

SELECTED AND ABSTRACTED PAPERS.

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CORRIGENDA.

Vol. cxvi. p. 308, second footnote, *for* "vol. cxviii." *read* "vol. lxv."

Vol. cxix. p. 272, second footnote, *for* "288" *read* "488."

Vol. cxx. p. 102, line 4 from bottom, *for* "52 tons" *read* "47·2 tons."

„ „ „ 3 from bottom and onward to the end of the sentence on p. 103, *should read* "the friction of the long rigid wheel-base of this engine on the frequent sharp curves rendered difficult the working of this maximum load without excessive wear and loss of power."

„ p. 381, lines 9, 10 and 11 from bottom, *delete* sentence beginning "for compression, &c." and *substitute* "For compression a steel tape is also used, a spring in the indicator moving the hand when the distance between the two plates is diminished by the compression of part of the structure."

Vol. cxxii. p. 318, line 16, *for* "R *v*" *read* "*k v*."

„ p. 320 „ 5, *for* "agreement" *read* "comparison," and *for* "closer" *read* "nicer."

, p. 328 „ 7, *for* "g" *read* "γ."

„ p. 330 „ 6, *insert* "for" *before* "the same."

„ p. 331 „ 18, *for* "latter" *read* "former."

„ „ „ 19, *for* "$\frac{3}{2}\,\frac{\phi}{2\,d}$" *read* "$\frac{3}{2}\,\frac{\phi}{2\,a}$."

„ Plate 5, the order of the three Figs. on the right of Figs. 11 should be reversed.

„ p. 439, line 8. The account of the pumping-machinery at Colombes is incorrect. This station now contains four double piston-pumps of the capacity stated. These pumps are driven by four engines of the Corliss type made by Messrs. Farcot, and there are eight tubular boilers. There is no centrifugal pump at this station.

, „ 21. There is no reservoir at the summit level.

p. 451, lines 5–11 and 35. In Mallet's articulated locomotives there are not two bogies; the boiler and the high-pressure steam-pipe are rigidly connected to the frames; but the front group of wheels, carrying the low-pressure cylinders, is properly described as a Bissel truck.

THE

INSTITUTION

OF

CIVIL ENGINEERS.

SESSION 1895-96.—PART I.

SECT. I.—MINUTES OF PROCEEDINGS.

12 November, 1895.

SIR BENJAMIN BAKER, K.C.M.G., LL.D., F.R.S., President, in the Chair.

ON the motion of the President, seconded by Mr. W. H. Preece, C.B., Vice-President, the Standing Orders in Section II., Clauses 2 and 3 of the By-Laws, respecting the mode of election by Ballot, were suspended in the case of His Imperial Majesty the German Emperor and King of Prussia, who was presented for election as an Honorary Member by the Council.

His Imperial Majesty was thereupon elected by acclamation an Honorary Member of the Institution.

The President then proceeded to deliver an Inaugural Address on taking the chair for the first time after his election:—

GENTLEMEN,—Two thousand years ago, certain Greek philosophers, turning aside, we may presume, from the serious duties of life for an hour's idle talk, as we are doing to-night, discussed the question of what was the proper course to pursue supposing, in case of shipwreck, chance threw a plank within one's own reach and away from that of a better man. Should we push the plank back towards the latter, or, failing that, would the crew of the vessel be justified in doing so? Varying answers were given to this difficult question by different philosophers, and, no doubt, in some form or another, we have all encountered the aforesaid plank and been somewhat perplexed to decide how to act. In the present instance, the plank in question is the one upon which I now stand, addressing you as President. I felt that it had merely slowly drifted in my direction, and finally reached me, owing to my long residence in Westminster and other chance influences, and therefore I did not hesitate to push it towards a better man—in fact, to the most distinguished man on the roll of the Institution—but the crew of the vessel, that is to say the Council, returned it to me;

and although I may doubt their wisdom, I cannot question their generosity.

Having accepted office, it is my duty, in accordance with ancient custom, to deliver an address of some kind. In the early days of the Institution, when technical literature was non-existent, a Presidential Address constituted a valuable medium for disseminating useful knowledge, and for that reason it was not only listened to, but read by engineers who, in the words of Cicero, looked for *multa lumina ingenii, multæ tamen artis*. At the end of the nineteenth century the conditions are, however, vastly different, for every newspaper now gives immediate and full publicity to all important engineering matters, as it is universally admitted that engineering activity in a manufacturing, maritime, and colonising country like our own, means general national prosperity. Presidential Addresses are, therefore, out-of-date productions which no one reads, though many are still written. For useful knowledge I refer members not to my Address, but to the technical journals and to the two thousand pages of "Proceedings" and "Abstracts" of one or two hundred Papers in foreign journals issued annually by the Institution; and, by so doing, I not only avoid adding to the burden of readers of the "Proceedings," but also accidentally disclaim any pretence of instructing the very experienced and critical audience I now have the honour of addressing.

Apart, however, from the ordinary scientific and practical work of the profession, which is dealt with so fully in the "Proceedings," engineers have so many interests in common, and their work is in touch with so many other branches of science, that there is no dearth of subjects for an hour's talk. Thus, having regard to the enormous increase in the number of our members and the keen competition of foreign engineers, attention might be directed to the importance of the Institution fostering by every legitimate means the spirit of comradeship amongst British engineers. Then the great question of the future of engineering is of vital interest to us all; and, as history repeats itself, we might glance backward to ascertain, if possible, the reasons why engineering is so essentially modern a calling, especially as regards steam and locomotion. Was it because the modern craze for rapid transit, often as purposeless as that of a record-breaking bicyclist, had not its equivalent in the past, or was it because our ancestors lacked the inventive power and mechanical ingenuity of the present age? and, above all, is the present position of our profession a mere mushroom growth, due to the exceptional stimulus afforded by the simul-

taneous re-organisation of the whole system of sea and land transport and of manufactories on the basis of steam power, or will the Institution of Civil Engineers continue to increase in numbers and in influence? Again, in connection with the same subject of the future of the profession, excursions might be made in the vast fields of physical science which constitute the borderland of engineering, for where the physical explorer advances the engineer settler quickly follows; or, abandoning the discussion of such wide questions, ample subject-matter for an hour's talk might be found in considering whether the Institution might not, by Committees of its Members other than the Council, do much useful work in settling standard clauses for specifications of different classes of work; standard sections of rails and details of various kinds, where the present meaningless lack of uniformity often leads to unnecessary delay and cost in manufacture. Some of the preceding subjects will be referred to in the present Address, but the limits of space make it impossible, of course, to do justice to any one of them.

Little need be said to enforce the importance of the Institution fostering the spirit of comradeship amongst its members, for the British engineer, like the British soldier, has to serve in all parts of the world, and, whether working at home or abroad, he is dependent for success upon the loyal co-operation of his colleagues. If he achieve fame, it is by being borne up on the shoulders of his fellow-workers, and not as the result of purely personal effort; for, although great engineering works are often popularly associated with one man's name, engineers well know that it would pass the wit of man to say how much of the merit was due to that particular man, and how much to the many others working with him and with the contractors.

The advantage and indeed necessity of engineers meeting to exchange past experiences, to discuss difficult problems awaiting solution, and to establish personal relationships, was recognised by our predecessors very many years ago. When, about the middle of the last century, a demand arose for increased facilities for internal communication by roads and canals, it necessarily happened that the men engaged in different parts of the country in projecting or carrying out such works occasionally met in the Houses of Parliament and the Courts of Justice of the Metropolis. On one of these occasions it was suggested to Smeaton that it would be well if some sort of periodical friendly meetings could be held where engineers might become personally known to one another; that, to use the somewhat quaint language of the time, "thus the

sharp edges of their mind might be rubbed off, as it were, by a closer communication of ideas, no ways naturally hostile; might promote the true end of the public business upon which they should happen to meet in the course of their employment, without jostling one another with rudeness, too common in the unworthy part of the advocates of the law, whose interest it might be to push them on perhaps too far in discussing points in contest." Smeaton at once approved of the suggestion, and on 15th March, 1771, that is to say, 124 years ago, a meeting took place at the "King's Head," Holborn, when it was agreed "that the Civil Engineers of this kingdom do form themselves into a Society, consisting of a President, a Vice-President, Treasurer, and Secretary, and other members, who shall meet once a fortnight on Saturday evenings, at seven o'clock, from Christmas, or so soon as any of the country members come to town, to the end of the sitting of Parliament; and it is further agreed that each member in town shall pay a forfeit of one shilling for being absent unless he is out of town." Under the name of the Smeatonian Society of Civil Engineers the Society so inaugurated has continued its work down to the present day, and still flourishes as a dining club, of which our distinguished Honorary Member, Field-Marshal Sir Lintorn Simmons, was one of the recent Presidents. During the first twenty years of its existence the Society could only boast of fifteen professional members; but as such names as Smeaton, Watt, and Rennie appeared in the list, the Society of Civil Engineers was, no doubt, as truly representative of the profession at that time as the Institution of Civil Engineers is in these later days. Our ancestors' method, therefore, of promoting comradeship was to dine together in a modest fashion some dozen times in a year, whilst we have one great feast annually. There is something to be said in favour of the old plan, and also of utilizing our fine new building, if possible, for occasional social gatherings, since, although the ably conducted technical press may partially fulfil the original purpose of papers and discussions at this Institution, it is the latter alone which can foster the no less important spirit of comradeship which Smeaton sought to promote in the interests of engineers a century and a quarter ago.

Surprise was sometimes expressed at the number of men desiring to follow the profession of civil engineering; but surely it was the most natural thing in the world, for every thoughtful student must gradually realize that the animal and vegetable kingdom present numberless mechanical problems, and that the universe itself, indeed, may in many respects be regarded as a supreme

engineering work designed by an Infinite Intelligence. Descartes, as long ago as 1628, regarded the human body as a physical mechanism, the action of which would be largely explained by the laws of matter and motion; and although he could offer no better explanation of muscular contraction than that it was due to the forcing of a fluid called, for convenience, "animal spirits" into a number of elongated cells, of which the muscle was assumed to be built up, he was undoubtedly advancing in the right direction. We know now that the fibrillar structure is the contractile substance, and that a transformation of the potential chemical energy of the muscular substance into mechanical work by means of heat is the real "animal spirits." As a transformer of energy, the animal machine will often be twice as efficient as an ordinary steam-engine. In some parts of the world, where coal is dear and straw abundant, horse and engine alike feed on the produce of the cornfield; but whilst straw alone suffices for the firebox of the portable engine, the horse requires corn also, but the weight of food supplied to the engine will be two to three times that given to the living horse.

The narrow border-line which divides the engineer from the physiologist is well illustrated in an interesting autobiographical note of the late Prof. Huxley. "As a boy," he writes, "my great desire was to be a mechanical engineer; but the Fates were against this, and while very young I commenced the study of medicine. But though the Institution of Engineers would certainly not own me, I am not sure that I have not all along been a sort of mechanical engineer *in partibus infidelium.* I am now occasionally horrified to think how very little I ever knew or cared about medicine. The only part of my professional course which really and deeply interested me was Physiology, which is the mechanical engineering of living machines; and notwithstanding that Natural Science has been my proper business, I am afraid there is very little of the genuine naturalist in me. What I cared for was the architectural and engineering part of the business, the working out the wonderful unity of plan in the thousands and thousands of diverse living constructions, and the modifications of similar apparatuses to serve diverse ends."

The vegetable kingdom is no less suggestive of engineering problems than the animal kingdom to the thoughtful student. Every tree is, in fact, a vegetable pumping engine, but hydraulic engineers would be sorely puzzled to explain how the large quantity of water required to supply the evaporation from the extended leaf surface is raised heights up to 400 feet and above.

We know that the source of energy must be the sun's rays, and we know further that, in the production of starch, the leaf stores up less than 1 per cent. of the available energy, so that plenty remains for raising water. Experiments have shown that transpiration at the leaf establishes a draught upon the sap, and there is reason to believe that this pull is transmitted to the root by tensile stress. The idea of a rope of water sustaining a pull of perhaps 150 lbs. per square inch may be repugnant to many engineers, but the tensile strength and extensibility of water and other fluids have been proved experimentally by our member, Professor Osborne Reynolds, and by Professor Worthington and others. A liquid deprived of air entirely filling a glass vessel when cooled pulls on the vessel, and at last lets go with a violent click. Water has been so stretched nearly 1 per cent. of its bulk, and the adhesion of the water to the sides of the vessel and the amount of the tensile strength were found to be quite equal to that of good mortar. With ethyl alcohol the modulus of elasticity, both in tension and compression, was constant up to the ultimate tensile resistance realized of 255 lbs. per square inch. Many hydraulic engineers have, no doubt, lived and died without encountering anything in their experience suggesting that water and other fluids were capable of resisting a tensile stress of no insignificant amount; and yet, as long ago as 1663, the fact was known, for the Secretary of the Royal Society then wrote to the Governor of Connecticut that "what puzzleth and perplexith us is that water defecated from air remains suspended and doth not at all subside after the receiver hath been exhausted of air"; and, in the same year, the President was asked "to entertain His Majesty" Charles II. with the sight of quicksilver sustaining itself by tension at a height of 50 inches when the barometer was at 29 inches, "something else but equipondency of air being," it was truly said, "necessary to explain this odd phenomenon."

If we turn from the animal and vegetable kingdoms to the universe at large, we are at once brought face to face with a multitude of engineering problems. How many of us, for example, have not vainly speculated why the moon should always turn the same hemisphere towards us, as a governor-ball always turns the same face to its spindle. No true engineer would be content to accept such a coincidence as a matter of chance, but would seek some equivalent for the mechanical connection between the governor-ball and its spindle. It remained for our Telford Medallist, Professor George Darwin, to demonstrate, by a brilliant series of mathematical investigations on lunar and solar tidal

friction, that the friction of the tides raised by the earth in the moon when in a semi-plastic state, fully explains the present identity of the axial and orbital revolutions, and the equivalent of the mechanical connection sought for is thus at last found.

Enough has been said, perhaps, to justify the statement that, living in the world we do, it was no matter for surprise that so many intelligent and observant students desired to become engineers. Problems of vast interest still await solution in all branches of physical science and engineering, and the supply of labourers is practically unlimited. The number of engineers, therefore, both at home and abroad, will doubtless increase at a more rapid ratio than the work to be performed, and the competition will be more severe in the future than in the past.

The Council of the Institution have not been unmindful of the march of events, but have endeavoured by an insistence upon certain educational qualifications on the part of the students and of professional experience on the part of the applicants for membership, to admit no one to the Institution whom they thought might prove unworthy of comradeship with the great body of engineers, whose labours in the past have helped to keep Great Britain so well to the front in all matters affecting the material prosperity of the people. What may be the course of events in the immediate future as regards engineering progress no one can pretend to predict, but it has been well said that "the future does not come towards us, but streams from behind over our heads," and a backward glance thus may afford some useful hints and lessons as to the future.

An impartial survey of actions and events recorded in history will satisfy most people that in all ages there were to be found men no less intellectual and enterprising than ourselves, and that the demands of the time, whether warlike or peaceful, have always proved capable of realization. That "necessity is the mother of invention" is true of all ages, and the only difficulty in making forecasts is the strange way in which the aims of different generations vary. History teaches us that the ablest engineers and the shrewdest politicians fail utterly in interpreting correctly the first faint indications of some new demand of the public, and if they indulge in prophecies they seldom are true ones. The popular notion that some great advance is due to the brilliant inspiration of a particular genius proves, on closer examination, to be wrong, as the advance was merely the result of the operation of the ordinary laws of supply and demand, and the genius himself very

probably will have committed himself in writing to a sufficient extent to prove that he really was drifting with the stream rather than piloting the ship. On the whole, a study of history should, I think, prove stimulating and encouraging to young engineers, for it will show them that a good time may be close at hand, though the wisest may fail to foresee it, and that they need not be deterred from exploring a road because their predecessors have marked up "no thoroughfare," since some of the most brilliant discoveries in all ages have been made under such conditions.

Our ancestors were content to go through the world on land or sea at walking pace, whilst we must in all things accord with the spirit of the age, and go even for pleasure by rail, steamboat, or bicycle, at the break-neck speed which made the weary reporter of a sporting paper recently exclaim :—

> "Oh! what an age of wear and worry,
> When all the world is in a hurry!
> The useless task still undertaking
> Of record making, record breaking!"

Engineers may affect to deprecate objectless record breaking, but, at the same time, they need no telling that it is fortunate for them that the end of the Nineteenth Century is such a restless age, especially as regards locomotion, or many engineers' offices and manufactories would be idle.

It is probable that the first invention of an appliance to facilitate man's locomotion was due to the difficulty experienced in walking across snow. Dr. Nansen, the Arctic explorer, quotes a passage from an old Norse book on Greenland where a traveller says that, "although the flying dragons there met with are marvellous, it is even more marvellous to hear of men who can tame to their service strips of wood in such a way that a man who is no speedier of foot than his fellows when he has no shoes on his feet, can, nevertheless, as soon as he has bound these beneath his feet, outstrip the birds in flight, or the swiftest hounds, or even the reindeer itself." Certainly, no more trivial appliance than a strip of wood 8 feet long and 4 inches wide could apparently be invented, and yet a Lapp on "Ski" will go 140 miles in a day, where without them he could not go a tenth of the distance. With a modern cycle on the best roads three times that distance can be covered in the day, and yet, although one would have thought such an increase in the locomotive powers of man would have been invaluable in the old coaching days, the cycle is the creation of yesterday. The subject was discussed long ago, we know, for Dr. Johnson dismissed it sarcastically with the remark

to Boswell that "a man had only to decide whether he would go on his own legs alone or carry a vehicle along with him as well;" but the people of the time did not want to hurry, and so the cycle remained undeveloped until the present restless age.

Sir John Rennie, speaking as President of this Institution fifty years ago, said that when, by improvements in roads, carriages, and horses, and by shortening the stages, the distance between London and Edinburgh was performed in forty-two hours, "animal strength and endurance had reached its utmost limits, and if any improvement was to be obtained it was requisite to obtain it from a different source." He did not foresee that railway travelling would utterly demoralise his successors where speed was concerned, for not long ago Selby drove from London to Brighton and back at an average speed which would have taken him to Edinburgh in thirty hours instead of forty-two; and, what would have astonished Sir John Rennie still more, is that the journey has been done in the same short time by men on bicycles, which is even stronger evidence of animal strength and endurance. Man has done much to supplement his own natural speed and power, but he has done nothing in that direction for the horse. In the year 1673 paddle-wheel tugs worked by horses were in use both on the Thames and the Tyne for towing vessels, and it is on record that between Gravesend and London they would take a ship of 1,000 tons against stream at a speed of three miles an hour. No land vehicles similarly worked by horses carried "on board" appear to have been ever tried; but, in these days of high-grade steel, roller bearings, and india-rubber tires, it would be interesting to ascertain experimentally whether the performance of a sturdy little Shetland or Iceland pony, carried on wheels instead of on his own legs, could not be quadrupled, as that of his weaker master certainly has been.

If we direct our attention, for a moment, to the development of water transport, we find, as in the case of land transport, that quite apart from all questions of steam and electricity, it has been reserved for the present generation to make advances and improvements in the most primitive contrivances, which apparently might equally well have been made thousands of years ago. The aborigines of America and many races of mere savages early found out the advantage which the lightness of birch bark canoes gave them in war or in the pursuit of game, and paddles of all kinds were developed. How, when speed was a matter of life or death to them, did they not ages ago give up paddles and take to oars, and find out, as our record-breaking rowing men have done,

that, by the simple contrivance of sliding seats, an advantage is gained which, other things being equal, renders the result of a race a certainty? Again, why, when sailing ships had been in existence thousands of years, did Columbus set off to discover America in a carved and decorated 70 feet long sarcophagus which could neither sail nor weather a storm, instead of in a Clipper ship? The men who built the "Santa Maria" for Columbus, the "Royal Harry," and many larger vessels, were quite capable as artificers of constructing with the same materials and implements Clipper ships of from 500 to 900 tons such as astonished the world in the historical race from China to London in 1866. At the end of May in that year, five vessels, the "Fiery Cross," "Ariel," "Taeping," "Serica," and "Taitsing," left Foo-chow-foo laden with tea for the race home. With varying luck, the different vessels, at times covering 328 miles in a day, proceeded on their long course, and in the Channel the "Ariel" and "Taeping" sighted each other for the first time since leaving China, and off Plymouth were racing neck and neck with every stretch of canvas set. Finally, after about a three months' race, the "Ariel" finished ten minutes ahead of the "Taeping," having left China twenty minutes before her; the "Serica" arrived the same day, the "Fiery Cross" the following day, and the "Taitsing" the day after. Allowing for the difference of time in starting, three of the vessels did the passage in 99 days, and the two others took two days longer. Here there was no help from steam or steel, but the old materials, wood and canvas, sufficed for the work.

As so many advances might have been made in the direction of rapid transit by sea and by land by the skilful but illiterate mechanics of many centuries ago without any aid from scientific research, it would appear probable that the reason for the solution of the problem being so long deferred was that wars, revolutions, and great social changes occupied men's thoughts in former times, and there was not that unceasing struggle for commercial supremacy and material advantages which is so characteristic of the present century. Necessity, therefore, did not give birth to Invention.

When the illustrious James Watt directed his attention to the improvement of the steam-engine, the Seven Years' War had just ended, and the country was weary of foreign and civil strife, and longed for the advancement of its material prosperity. The establishment of the first ironworks in Glasgow, the deepening of the Clyde in 1760, and the opening of the Bridgwater Canal,

would naturally lead an original mind like Watt's to foresee some of the vast consequences of the development of manufactures and inland transport, and the hopeful spirit of the times acted as a stimulant to many other inventors. Thus, whilst Watt decided upon separate condensation in 1765, and patented improvements leading up to the double-acting rotary beam-engine in the years 1769, 1781, and 1782 respectively, we find that the years 1767, 1768, 1779, and 1784 successively saw the births of Hargreaves's spinning-jenny, Arkwright's spinning-machine, Crompton's mule, and Cartwright's power-loom. Without these and similar improvements, there would have been as little demand for Watt's engine as for Lesseps' Suez Canal before the development of steam-navigation in the British marine.

It is difficult for us to realize that in the years referred to England was chiefly dependent upon her American Colonies and Russia and Sweden for the supply of bar-iron, and that Birmingham was popularly regarded as the centre of the iron industry of Great Britain. The latter fact is illustrated by an ambitious poem of the time, entitled "Panegyric upon Iron":—

> "Soon o'er thy furrowed pavement, Bremicham!
> Ride the loose bars obstrep'rous; to the sons
> Of languid sense and frame too delicate
> Harsh noise perchance, but harmony to thine."

In 1751 application was made to Parliament for the admission of bar-iron duty free from our own Colonies, and an Act was passed authorising this so far as the Port of London was concerned, but admitting pig-iron only at other ports. A clause, however, was inserted in the Act prohibiting the carriage of American bar-iron beyond 10 miles from the City, with the acknowledged object of preventing the wagons which brought manufactured ironwork from Birmingham and Walsall returning home laden with bar-iron, and so furthering competition with London ironworkers. There was to be no free trade, in fact, between London and Birmingham. As, however, the dearness of coals and the heavy duties thereon prevented the setting up of manufactories in London, the Act was practically a dead letter, and another appeal to Parliament was made in 1755. In the petition the method of manufacturing bar-iron was explained to Members of Parliament in these words:—"The first process is to refine the iron from the ore, and the only fuel proper for this operation is charcoal. The next process is to meliorate the iron still by means of a charcoal fire to render it malleable and draw it into bars under the strokes of a great hammer. After that the iron comes into the hands of

the manufacturers, the use of wood charcoal is from thenceforth entirely laid aside, and they perform all their operations with Pit coal." It was further stated that Swedish iron was charged with £3 12*s.* 6*d.* per ton export duty on leaving Sweden, and £2 8*s.* 6*d.* import duty on arriving in this country; that the Swedes had limited the production, and the Empress of Russia forbidden the erection of more ironworks; and that "the present alarming connection of Russia with France should rouse us the more to turn our thoughts towards our Colonies for supply."

On the very eve, therefore, of Black's discovery of the "latent heat" of steam, and Watt's practical application of the theory to the steam-engine, Parliament and the public at large had not the dimmest perception of the momentous revolution which was to change Great Britain from a suppliant buyer of bar-iron from abroad to the manufactory of all kinds of iron and steel for the world. Indeed, some years after Watt's first patents, Parliament was asked to grant bounties on American pig-iron; but in 1783, that is to say a year after his patent of the rotary steam-engine, Sir John Dalrymple, a Scotch ironmaster, published a pamphlet in which he expressed the opinion that Watt's invention will give the command of the iron trade of the world to Great Britain, and take it for ever, or, at least, as long as the industry and liberty of Britons remain, from the Northern kingdoms and from America, because Britain is the only country in which seams of coal, iron ore, and limestone are found in the same field, and of short water-carriage to the sea." This opinion, however, was not the general one, for Watt's twenty years' work had, up to that time, been treated with indifference both at home and abroad. Later on the swing of the pendulum caused an error in the opposite direction, and the public attributed to Watt's foresight and genius results which we who see things in their true perspective know to have been the ordinary consequences of the law of demand and supply. Up to the middle of the last century such manufactories as existed were established where water-power was available, and naturally no demand existed for the more expensive steam-power. At certain places where water at times ran short, Smeaton and others adopted the atmospheric pumping-engine to supplement the supply of water to the overshot wheels. As the population of the country increased, and commercial questions for the first time began to be regarded of paramount importance, the improvement in trades and manufactures rendered it necessary to find other sources of power than water; and when the necessity arose, the engineers of the time, of course, found no difficulty in developing the hitherto

neglected steam-engine, just as electrical appliances of all kinds are now being developed in response to present demands.

Even now we occasionally find writers and public speakers referring to the steam-engine and its momentous results as the outcome of a brilliant inspiration of Watt's transcendent genius, for which the world would have vainly waited had not Watt been born. If there is one lesson that history teaches us beyond all question, it is that when the time comes the man is never found wanting. Watt himself never claimed to be either a scientific or mechanical genius. Writing in 1808, he said, "My soul abhors calculations, geometry, and all other abstract sciences"; and again he said, "I am not over-anxious about fame, yet I am more proud of the parallel motion than of any other mechanical contrivance I have ever made." Judging from present engineering practice, we may think that this was an unfortunate foundation for Watt to base his future fame upon, as the parallel motion is of no interest now except as a geometrical problem.

There are two points in connection with the early history of the steam-engine which have always appeared to me absolutely astounding and incomprehensible: first, that any mechanic of the period, accustomed all his life to work his lathe by treadle, connecting-rod, and crank, should imagine that the application of the crank to a single-acting steam-engine was an "invention," and actually secure a patent for the same; and secondly, that a man of Watt's ability and experience should endeavour to circumvent that patent by himself patenting that fearful "back-lashing" nightmare, the so-called "Sun and Planet" motion. It is not less remarkable that Smeaton about the same time, when reporting on a paper submitted to the Royal Society, of which he was a Member of Council, condemned the crank and fly-wheel because of the imagined difficulty arising from the stopping of the heavy reciprocating mass at each end of stroke, and expressed the opinion that the use of the fly-wheel would be a greater encumbrance to a mill than a water-wheel to be supplied with water pumped up by an engine.

There is nothing, therefore, in the history of the development of the steam-engine, or in the brilliant career of Watt, to lend support to the theory that the progress of mankind is dependent upon the periodic appearance on the stage of some actor of inspired genius, but, on the contrary, many illustrations are afforded of the fact that men of the highest ability and experience are as apt to be wrong in their forecast of events, and in their estimates of the relative importance of their own or other

persons' investigations or inventions, as are those of humbler powers.

The latter historical truth applies not only to engineers, whose necessarily close attention to detail might make short-sighted as regards distant events, but to statesmen of the highest standing, whose training and study of mankind at large, one would think, should qualify them to make a reasonably accurate forecast of the demands of the public in the immediate future. Take, for example, some incidents in connection with land and water transport. We Londoners often complain of the want of system in the arrangement of the railways and their terminal stations in and around the Metropolis, which necessitates our performing long journeys in cabs to get from one railway system to another. That this difficulty exists, arises, I feel sure, chiefly from the want of foresight of no less able a statesman than Sir Robert Peel, for in 1836 a motion was proposed in the House of Commons that all the Railways Bills seeking powers for terminals in London should be referred to a special committee, so that a complete scheme might be evolved out of the numerous projects before Parliament, and that property might not be unnecessarily sacrificed for rival schemes. Sir Robert Peel opposed the motion on the part of the Government on the grounds that "no railway project could come into operation till the majority of Parliament had declared that its principles and arrangements appeared to them satisfactory, and its investments profitable. It was a recognised principle in these cases that the probable profits of an undertaking should be shown to be sufficient to maintain it in a state of permanent utility before a Bill could be obtained, and landlords were perfectly justified in expecting and demanding such a warranty from Parliament." In this instance, incalculable injury was unintentionally inflicted upon Londoners by not having a grand central exchange station for through as distinguished from terminal traffic in the Metropolis, and events have shown how false was the assumption that the passing of an Act implied any warranty as to the financial prospects of a railway.

So much as regards the construction of railways. Now let us consider for a moment the question of speed on railways. In 1857 Lord Brougham, as President of the Social Science Congress, read a paper on Railway Accidents, advocating the State limiting the maximum speed on railways to twenty-five or thirty miles an hour. As to danger, he said, "It is better to rely upon the nature of the thing itself, and no testing nor any authority is wanted to ·· ᐧ rapid motion *must* be dangerous. It is undeniable that

a very great majority of those who travel would prefer the security, and declare themselves satisfied with a moderate speed—with going from London to York or Liverpool in eight hours, and to Edinburgh in sixteen." In the light of the recent "Race to the North" was ever forecast more wrong? As regards speed, the public are satisfied with nothing less than double Lord Brougham's proposed statutory maximum, and as regards accidents to passengers they are not as numerous now as in the old coaching days, notwithstanding the incomparably greater number of passengers.

Equally fallacious forecasts were made by the highest authorities as to the probable commercial results of undertakings. Thus in the same year that Lord Brougham made the preceding statement, Lord Palmerston, speaking in the House of Commons on the question of the Suez Canal, said: "I am not much out of the way in stating this to be one of the bubble schemes which are often set on foot to induce English capitalists to embark their money upon enterprises which, in the end, will only leave them poorer whomever else they may make richer;" and Robert Stephenson, following him and addressing the House as an engineer, said: "Commercially speaking, I frankly declare it to be an impracticable scheme." Stephenson, however, was not the only President of this Institution who has failed as a prophet, for Telford, in 1829, reported adversely on the partly made Liverpool and Manchester Railway, saying that the use of horses had been rendered impracticable by the introduction of two sets of inclined planes which must be worked by locomotive or fixed engines, and he could not take it upon himself to say whether either would fully answer in practice.

All these mistakes, however, merge into insignificance when contrasted with the gigantic blunder the whole civilised world made in the early half of this century, in assuming that the efforts of the engineer would effect an immediate change in the long-inherited savagery of mankind and inaugurate a period of universal brotherhood and peace. For some few years events appeared to justify the anticipations. Dr. Lardner, writing sixty years ago, said that "it was due to the steam-engine that Reason had taken the place of Force, the pen superseded the sword, that war had almost ceased upon the earth, and that the differences which inevitably arose between people and people were for the most part adjusted by peaceful negotiations." How cruelly these anticipations have been falsified by subsequent events is only too well known to the present generation. No other age has witnessed so gigantic a fratricidal struggle as that between the Northern and

Southern States of America, nor so sudden and overwhelming a descent of armed enemies on its soil as France experienced in 1870; and in both cases the work of devastation was facilitated, and the horrors of war were intensified, by the labours of the civil engineer.

Doubtless in all ages the civil and the military engineer co-operated, but at the present time their several duties are so closely interwoven that it is almost to be regretted that, following the example of the Engineering Congress at Chicago Exhibition, the word "Civil" has not been eliminated from the title of our Institution.

In mediæval times fortifications often formed an integral part of important bridges, and the civil and military engineer were thus early thrown together. Thus the Ponte di Castelvecchio at Verona, a three-arched bridge built in 1354, with a central span rather larger than that of London Bridge, had loopholed parapets for its entire length, and fortress towers rising to a height of more than 150 feet on one bank of the river. The Ponte di Trezzo, constructed by Barnabo Visconti between 1370 and 1377, crossed the Adda where it was deep and rapid by a single arch of no less than 237 feet span and 68 feet rise, and the roadway was entirely covered in like the galleries of a fort to unite the castle on the right bank of the river with the fortifications on the left. The word "hoarding" is one of the many evidences of the bond between the soldier and the civilian, for it was the name in use by the Normans 800 years ago for the temporary wooden screens erected on their castles when repelling an attack.

A new bond of union between the civil and military engineer arose with the introduction of railways. In times of war railways play an all-important part, and thus the civil engineer in laying out a line has often to take into account strategic considerations no less than prospective commercial advantages, good gradients, and minimum cost of construction. The civil engineer also has had to lend his aid in the construction of railways during the actual progress of a campaign. The first instance of the kind occurred in 1854, when Peto, Brassey, and Betts, assisted by the directors of every railway in the kingdom, who placed all their stores at the service of the nation, succeeded in sending out to the Crimea twenty-three large steamers with men, horses, locomotives, and stores, and within the first twelve days of arrival they had laid seven miles of line. The soldiers previously engaged in handing shot and shell to each other, standing knee deep in mud the whole way from Balaclava to Sebastopol, were thus freed from

a deadly slavery, and those in camp were for the first time properly fed and clothed. Before the completion of the siege 39½ miles of line were laid around Sebastopol and worked by seventeen locomotives, but after the first "spurt" the progress was slow, as the indispensability of adequate railway transport for the purposes of modern warfare was imperfectly realized in 1854. If it had been otherwise, indeed, the Crimean War could not have taken place, for with sufficient railways the Russians would have poured enough troops into the Crimea to have overwhelmed the Allied Forces.

Very different were the views held by the Russians in 1877, when they made their dash at Constantinople. "Within little more than four months," to quote Captain Sale, R.E., "during actual war, and whilst military operations were in the fullest activity, over 230 miles of new railways were made, the earthwork of some 75 miles of additional line well advanced, a rolling-stock of 120 locomotives and 2,150 wagons and trucks manufactured and delivered, and a steam ferry over the Danube provided for, and all this in addition to a number of minor railway works." The most important of the above lines was that from Bender to Galatz, 189 miles in length, with gradients generally not exceeding 1 in 100, but for a short length 1 in 56. On the whole the country was favourable for construction, but near the Danube and elsewhere embankments had to be built strong enough to resist the action of waves, and across the Pruth an opening bridge was necessary, which, with all other bridges, was built of timber. Labour was scarce, but, notwithstanding that and many other drawbacks, the line was completed in 100 days, barely more than half of which were fair working days. The wagons were conveyed across the Danube in iron barges, manipulated by chains, as, of course, the stream was too wide and deep to admit of the construction of a bridge during the progress of the campaign.

In recent years it has been accepted as an axiom by military authorities, that to conduct operations of importance with the best advantage in a country unprovided with railways, it is necessary as a first step to construct a light railway on the line of advance. This is quite in accord with the practice of modern railway contractors, who commence operations by laying down with all speed a light "overland route" railway from one end of their work to the other. The administrative authorities are of opinion that the requirements would be met by a railway of 2-foot 6-inch gauge, with 36-lb. rails, worked by four 20-ton locomotives, and that the total weight for shipment of 25 miles of railway and

sidings, complete with rails, sleepers, bridgework, repairing-shop, and all necessaries, including rolling-stock, would be a little over 5,000 tons, which could be carried in two comparatively small vessels, as compared with the "twenty-three large steamships" requisitioned in 1854 for the nine miles of Crimean line already referred to.

The transport arrangements for a large moving mass of men may be regarded as the joint work of the military and the civil engineer. In the advance of the Prussians towards Paris, each man carried with him one day's ration for immediate consumption, with three days' reserve rations, and accompanying each Army Corps were five Provision Columns, carrying provisions for four days more, including driven cattle. For the supply of the provision columns magazines were formed where possible, and on the average 600 hired wagons were required to convey the provisions from the magazines to the five columns. At this stage railways come on the scene, for by railway transport alone could the vast supplies be forwarded for filling up the magazines. The long line of transport wagons necessary even for a single Army Corps will be obvious when it is considered that, allowing for the inevitable opening out on march and taking equal numbers of wagons and carts, a line of 100 vehicles would stretch about a mile in length. But all these long lines of wagons require protection, which is an element engineers seldom have to reckon with in their operations. The country round would be scouted by cavalry, and there would be an advanced guard of cavalry and infantry, and possibly artillery, ahead of the wagons. In the case of a convoy by rail, a pilot-engine with advance guard often precedes the train to reconnoitre the country and to see that the permanent way and works have not been displaced. In case of attack, the escort descends and opens fire, assisted occasionally by field-guns mounted on ordinary railway trucks, protected by an easily-constructed armour of rails from the railway-sidings—an arrangement experience has shown to possess sufficient stability to admit of 25-pounder guns being fired at any angle to the line of railway. When account is taken of the imperfect knowledge of the district which the officers of the escort must frequently possess, and the small time available for consideration when a convoy is attacked by the enemy, we must all readily admit that the mere transport arrangements of an army on active service constitute a work of greater difficulty and anxiety for our military brethren than most of those we are called upon as civilians to undertake.

In our own country railways form such a close network that they must inevitably play an all-important part in any scheme of defence. To assist the military authorities in transport matters the "Engineer and Railway Volunteer Staff Corps" was founded, in which many members of this Institution, railway engineers, general managers of our chief railways, and leading contractors, hold Her Majesty's commissions, and, with their aid, the problem of massing large bodies of troops rapidly at any required point has been worked out in all detail, and will be given effect to in case of necessity.

But, important as are the modifications necessitated in warlike operations by the inventions and labours of the railway engineer, it is beyond dispute due to the mechanical engineer that every detail of combat on land or sea, and every preparation for it, have been entirely revolutionised during the life of the present generation. Each successive improvement in armaments effected by the mechanician has necessitated a change in tactics. When the flint-lock musket, with bayonet at end, superseded the bandolier and pike, the infantry for the first time adopted the "thin red line" formation so celebrated in English history. Now, with the magazine-rifle, a loose formation and a closing up for the final rush to carry a position at any cost of life has become the necessity.

As regards the Navy, the question of guns and armour has occupied the continuous attention of engineers. Since the time of the Crimean War the charges of powder have increased from 16 lbs. to 900 lbs., the weights of projectiles from 68 lbs. to 1,800 lbs., and the energies developed from 1,100 foot-tons to 62,000 foot-tons. Further changes are rapidly following the introduction of the more powerful smokeless powders. The name which in this country will ever be honourably associated with improvements in Ordnance is that of our Past President, Lord Armstrong. When in 1882 he described the then condition of the armaments of the country, he wisely deprecated any idea of finality, and pointed out that the ceaseless labours of engineers must ever lead to progressive changes both in guns and armour. Events have amply justified his warning. Since 1882 the Navy has been entirely re-armed. The Armstrong muzzle-loading wrought-iron coil gun has been superseded by the breech-loading steel gun of vastly increased power; and whilst in 1882 Lord Armstrong told you that there was not "any material difference in regard to rapidity of fire between breech-loading and muzzle-loading guns, nor would a superiority in this respect on either

side be of much value," he himself has since inaugurated the system of "quick-firing" guns as an indispensable adjunct of every war-ship, although in complexity of breech mechanism—which was what Lord Armstrong dreaded in 1882—they compare with the old muzzle-loader in much the same way as would a watch with an hour-glass. The escape of powder gas at the breech was one of the difficulties at first encountered, and what engineer would have been bold enough to predict that gas at a pressure of over twenty tons per square inch, and at a temperature above that of molten steel, would be effectually sealed by a canvas-covered pad of asbestos fibre soaked in tallow, and yet that expedient has proved to be a complete solution of the difficulty.

Civil engineers are often reproached for devoting their ingenuity to the devising of man-killing appliances, but with little reason; for whatever the abstract principles of Christianity may be, no reader of the newspapers can fail to see that reliance is not placed upon those principles in practical politics, but that it is always taken for granted that if we happen to possess something which another nation covets, that nation will, sooner or later, attempt to take it from us, unless we are strong enough to make the attempt hopeless. When, therefore, our military authorities expressed a desire to quadruple the man-killing power of rifles and field-guns, as compared with what prevailed at the time of the Franco-German war, engineers and mechanicians spared no pains to devise what was wanted, and the magazine-rifle and modern shrapnel-shell were the outcome of their ingenuity. Lord Wolseley has said that a rich nation like ours may have a long purse; but under the conditions of modern warfare, there is no time to draw the purse out of the pocket. This is due to the work of engineers in the provision of railways, steamboats, and telegraphs, and in the introduction of guns and other appliances of too great complexity to be obtained on demand, for it takes as long to construct a big gun as to build an Atlantic liner. The members of this Institution thus being largely responsible for the altered conditions of warfare, will doubtless in the future, as in the past, zealously aid the Army and Navy in the defence of what is ours, and more than that is not required, for we covet no other nation's possessions.

But engineers are not only reproached for making war more terrible, but also for rendering life less enjoyable by reason of what uninstructed critics call the hideousness of the manufactories and other works with which the world is now getting covered. Thus, one of our popular novelists, referring recently to an old

tower in Italy, with a conscious "dig" at engineers, says: "It belonged to that age in which men knew so well how to unite the useful and the beautiful, to harmonise the lovely with the formidable, and how to use the sports of peace to hide the strength of war. On the site of the tower there is now standing the chimney of a factory, belching forth its stinking vapours to the sullied waters and the outraged heavens. The change is called progress." Doubtless, in this instance, Italy is artistically poorer by the loss of a picturesque old tower, and similarly some of our most lovely scenery has been hopelessly disfigured by collieries and their squalid surroundings; but, on the other hand, owing to the labours of engineers, travellers can visit the unaltered peaks and valleys of the Alps, and even of the Rocky Mountains, in less time and at less cost than they could formerly enjoy the natural beauties of their own country. As regards the structures themselves, much of the abuse formerly lavished by art-critics upon the work of engineers was due simply to the complete ignorance of the critic as to the functions and purpose of the object of his criticism, and his inability to see therefore, from lack of previous experience, that the general form and details, though differing from his preconceived notions, were fitted for their purpose and expressive of the same. In the early days of bridge building, for example, art-critics were familiar with arches and suspension bridges, and everything not of those types was unfamiliar and unlovely. They could see beauty and fitness in the masts and rigging of a ship, although they were nothing but mere struts and ties arranged for a special purpose, without any aim at artistic effect, but they could see none in a lattice girder, however well-proportioned, because of unfamiliarity and ignorance.

Engineers are weak enough occasionally to attempt to conciliate such critics, instead of gradually educating them by the construction of works scientifically and economically adapted to their purpose. We thus find so-called ornaments in the form of stars, scrolls, and finials plastered upon and about some of the earlier lattice bridges; we also see ancient marine engines with Gothic framework, constituting in effect an agglomeration of bent struts and crooked tie-rods in cast iron, with no bar in the direction of the stress; pumping and mill engines, with columns modelled after those of some Greek temple or Egyptian tomb, and early locomotives with a brazen American eagle perched on the steam dome, and apparently shrieking with rage because he had alighted on so warm a spot. Commercial competition has aided good taste in doing away with all such monstrosities; and if engineers but

honestly persist in designing works of a simple and scientific type, art-critics will gradually become educated up to the necessary standard to enable them to detect the beauty of fitness in such works, as with longer experience they now see it in old timber-framed brick and tile cottages, fishing boats, windmills and water-wheels, and many other things whose designers gave small thought to anything but the purpose they had to fulfil and the nature of the materials they had to employ.

We need trouble ourselves little, therefore, either about the charge of want of human sensibility in devising war appliances, or of Vandalism in the occasional necessary injury to objects of archæological interest or natural beauty, for our aim is the general good, and we are no more open to such charges than is a policeman who knocks down a burglar, or a surgeon who amputates a limb to save the patient. We have generally taken it for granted that everyone acknowledged the benefits conferred upon humanity by engineers in facilitating communications, cheapening production, and raising the condition of the labourer, but we are living in a sceptical age, and no less an authority than a most distinguished Professor of History at Oxford University has questioned the truth of any such assumption. In his inaugural Address in 1892, the late Professor Froude said that he failed to see much evidence of progress in the nature of men, though there is more knowledge of material things, and that "even in the outward essentials of food and clothing and housing it is not certain that the mass of mankind in the present generation are better off than their forefathers. The condition of the people in mediæval Europe was not as miserable as is pretended, and the distribution of the necessaries of life was not as unequal as it is at present. If the tenant lived hard, the lord had little luxury. Earls and countesses breakfasted at five in the morning on salt beef and herring and a draught of ale from a black-jack. Lords and servants dined in the same hall and shared the same meal, and, as for dress, plain leather and woollen served for all ranks except on splendid ceremonials."

Engineers will probably still be of the opinion that the mass of mankind are much better off than their forefathers as regards the essentials of food, clothing, and housing; and further, that the independence of the labourer and mechanic has been greatly advanced in consequence of the vastly increased demand upon their services due to engineering work. Even so recently as four years after the opening of the Liverpool and Manchester Railway, certain Dorsetshire labourers ventured to form themselves into an elementary form of trades union, and were sentenced to seven years'

penal servitude for their pains. At the present time many municipal contracts provide for payment at trades union rates, whereas a couple of centuries ago the magistrates fixed the rates at Quarter Sessions according to the price of food and clothing, the average being about 1*s.* per day for carpenters and masons, 8*d.* for labourers, and 4*d.* for women, with a deduction of 1*d.* per day during winter months. The hours of labour for all classes were from 5 A.M. to 8 P.M., with two and a half hours off for meals and rest. The penalty for an employer departing from these regulations in many places was ten days' imprisonment and £5 fine; and for a workman, twenty-one days' imprisonment. No man in search of work could beg without a licence, the statutory penalty for a first offence being whipping at a cart's tail "till the body be bloody by reason of such whipping," whilst the second offence meant the loss of an ear and two days' scourging, and the third one hanging. These facts hardly bear out the Professor's implied denial of any change in the nature of man, or in the distribution of the necessaries of life.

A couple of centuries ago, in a carefully-prepared statement by the *Lancaster Herald* of the average estimated incomes of different classes of population from peers to crofters, there was included a class styled "followers of the liberal arts and sciences," which would doubtless comprise engineers. It is interesting to note that the average income per family of this group is given at £60 per year, as compared with £400 for eminent merchants, £154 for lawyers, £72 for eminent clergymen, £45 for shopkeepers and farmers, £38 for highly-skilled artisans, £15 for labourers, and £6 10*s.* for crofters or cottagers. As the family of a scientific gentleman in those days of universal marriage was estimated to average five in number, the expenditure per head must have been included within the sum of £12 annually; but fortunately, as the old chronicler quaintly remarks, families were smaller in London "from a greater intenseness on business, and from the unhealthfulness of the coal smoake." So our predecessors in Great George Street were somewhat better off than these figures indicate.

It would be a great mistake to assume that the relatively low earnings of past time were compensated for by the lower cost of necessaries. Owing to the want of roads, railways, and canals, the price of corn varied widely throughout the country, and 200 years ago it ranged from 22*s.* to 76*s.* per quarter, or much above the present average price. Beef and mutton cost from 3*d.* to 4*d.* per pound, and were therefore beyond the reach of the poorer classes, whereas at the present time, owing to the work of engineers in

providing fast steamers and refrigerating apparatus, about five millions of frozen sheep are purchased annually in New Zealand, Australia, and the Argentine for 2*d*. per pound, and delivered on the market here for another 2*d*. Beef is dealt with in the same way, so that, having regard to the current rate of wages, beef and mutton need no longer be considered mere occasional luxuries for any class of the community.

Even in our own Smeaton's time, the position of the workman was little better than it was a couple of centuries ago. When Smeaton was consulted about the building of the bridge across the Tay at Perth, a London contractor was sent for who, after taking borings and receiving instructions to commence at once, said he must go back to London to bring three of his men skilled in pier-work, but would return at once and, "with God's help," would do the double journey in six weeks. As the journey was so long, and the time of employment only five months, he would have to offer two of the men 14*s*. and the third 7*s*. per week. It is interesting to note that the price of masonry in hydraulic lime was 5*s*. per cube yard, with 8*d*. per super foot extra for the facework, whilst the price of some cast-iron pipes was £16 per ton, showing a change in the relative prices of the two classes of work of more than ten to one during the past century. Even twenty years subsequent to the above, the wages in Scotland were 8*s*. for a smith and 5*s*. for a striker, and at that time the coach fare from London to Edinburgh was £5 5*s*., allowing 28 lbs. of luggage, with 6*d*. per lb. for extra, so that a trip to London and back by coach would absorb six months of a smith's earnings instead of little more than a week's, as at the present time—surely a wondrous transformation.

On the whole, a study of the past, if we except the terrible scourge of war, is conclusive as to the enormous material and moral progress which has accompanied the development of engineering works. Such study, as I have already indicated, may reasonably make young engineers bold and old ones modest; for, although the latter may justly claim to have done much for the benefit of humanity, they have often failed dismally as prophets, and in their superior wisdom have frequently discouraged young engineers from following a line of investigation which has subsequently led to results of unlooked-for value. Although the wisest cannot foresee the demands of the next age, history shows us that new wants constantly arise in an increasing ratio, and that engineers will always find work in meeting them. Engineering certainly is no mushroom growth, and the Institution must

continue to increase in numbers and influence. When our coal-fields are exhausted, doubtless other sources of power will be in full operation, and the recent development of electrical engineering indicates the advisability of young engineers watching closely what physicists are doing in the borderland of engineering, not only the better to enable them to understand the work they have in hand, but also to qualify themselves to take first advantage of any new demand of the public.

Our revered past Member of Council—would I could say Past President—Lord Kelvin, has well remarked that "engineers must and do patiently observe and discover by observation properties of matter and results of material construction, but deeper questions are always present and always fraught with interest to the true engineer." Our late Honorary Member, Professor Tyndall, has further told us that to really understand things we must learn "to see the invisible as well as the visible in Nature, to picture with the eye of the mind those operations which entirely elude the eye of the body, to look at the very atoms of matter in motion and at rest and to follow them forth, without ever losing sight of them, into the world of senses." We, as practical engineers, are constantly encountering phenomena which we cannot explain, but reluctantly have to relegate to the class known as "mysterious." The true engineer, however, will never rest satisfied until he has at least discovered the laws connecting the phenomena, although exact positive knowledge regarding the phenomena themselves may be beyond human attainment. Many things, doubtless, appear "mysterious" to the engineer because the assumptions made by him in ordinary work are rough generalisations rather than particular truths. Shipwrecks would similarly appear "mysterious," if we ignored the waves because their height was apparently negligible in comparison with the diameter of the earth. We know that iron expands with heat, and jump at the conclusion that this expansion will go on from the solid to the molten state; and when we see a piece of solid cast iron immersed in molten iron rise to the surface and float like ice on water, we term it "mysterious," whereas the flotation is a necessary consequence of the complicated molecular changes going on in the iron. We assume, again, that a red-hot bar of steel will cool gradually and steadily, and then style the curious wriggling of a flat-bottomed steel rail when cooling "mysterious," whereas, as a matter of fact, a red-hot steel bar does not cool gradually, but cools down to a certain point, then becomes hotter again, after

which it resumes the cooling process only to make further oscillations of temperature which affect the thin flange and the thick head of the rail at different times, and so cause considerable cambers in opposite directions before the rail ceases to be red-hot. We know now, from the researches of physicists, that changes in thermal condition and changes in the arrangement of the metallic and other elements, and even of the atoms in the molecule, occur simultaneously, and that between the temperatures of 1,200° to 1,600° there are several so-called critical points where the cooling process is arrested, and thus the wriggling of the rail ceases to be "mysterious."

The effects of hardening, tempering, and annealing, familiar to the world doubtless for several thousand years, have only recently been partially lifted out of the class "mysterious" by researches of a like nature to the preceding. There are many other "mysteries" of an analogous kind waiting to be cleared up. We should like to know, for example, what is going on month after month in a hardened steel armour-piercing projectile which frequently leads finally to a violent disruptive explosion of the mass, and also what causes a sword to lose temper by lapse of time, whilst the edge becomes sharper. Why, again, should the tough and flawless bar-iron suspension links, which had carried the Hammersmith bridge successfully for over sixty years, snap in two by the dozen during simple transport to Edinburgh, although in every case the halves of the broken links, on being thrown down 300 feet from the top of the Forth Bridge on to the rocks below, bent like a corkscrew without fracture? Practical engineers have been aware for forty years past from Fairbairn's experiments that at temperatures at 60° and of 320° the strength of wrought-iron was practically constant, whilst at 30° the strength was slightly increased; but until Professor Dewar's recent researches they could never have conceived that when immersed in liquid air at a temperature of − 320° the strength of iron wire would be raised from 34 tons to 62 tons per square inch. The chemical constituents of iron and steel do not change, but the molecular arrangement and intercrystalline cohesion must change, and it is to mathematical investigation and laboratory work rather than to practical engineering that we must look for an elucidation of the process.

Lord Kelvin, thirty years ago, directed attention to the fact that wires kept in torsional oscillation during the week behaved quite differently after their Sunday's rest, and it has been recently shown that, owing to the molecular settlement which occurs during rest in an overstrained bar, the modulus of elasticity will

rise 10 per cent. after three weeks' holiday. In practice probably every bar and plate in an engineering structure goes into it in an overstrained condition, owing to irregularities in cooling, cold-straightening, and other causes; therefore, although engineers profess to work out the exact maximum unit stress in a bar subject to a pull not coinciding with the centre of gravity of its cross-section to 1 per cent. accuracy, they well know that the result may be 100 per cent. wrong, owing to initial stresses in the metal. They also know that rivets would make very poor work if not put in sufficiently tight by hydraulic pressure to be overstrained during cooling, and there are countless contingencies in practical work which cause metal to be similarly overstrained, but with good ductile material experience has shown that the molecules apparently adapt themselves to this condition and no evil results. Doubtless, the molecular movement of adjustment would be "next to nothing" by actual measurement; but our thoughtful and practical Past President, Sir Frederick Bramwell, in choosing the "next to nothing" as the subject of his Presidential Address at the British Association, has warned engineers not to ignore small things, and, in doing so, he was following that very wise man, Lord Bacon, who, nearly three centuries ago, remarked that "whoever will not attend to matters because they are too minute or trifling shall never obtain council or rule over nature. The turning of iron touched with a loadstone towards the poles was found out in needles of iron, and not in barres."

The instantaneous way in which a drop of oil flashes over a clean surface of water illustrates in a most striking manner the influence of infinitesimal forces, for the spreading is due merely to the excess of the tension of the surface separating the water from the air, as compared with the sum of the tensions of the surfaces separating the water from the oil and the oil from the air. A film $\frac{1}{30}$-millionth of an inch in thickness produces marked results, and yet to cover the whole 135 acres of painted surface of the Forth Bridge with a coat of that thickness would require less than a pint of oil. It would appear inconceivable that such a membrane could in any way affect the ocean in a storm, yet when in the winter of 1891 Admiral Cuverville's ship, the *Naïade*, was caught in a cyclone in the North Atlantic, and a greasy touch was given to the waters by rigging out two coal sacks, each filled with about 11 lbs. of tow and 1 gallon of colza oil, which latter required renewal only every six hours, the scientific and trained observers on board the French warship reported the result to be a remarkable practical success, the oil taking effect upon the dan-

gerous breakers due to horizontal translation produced by the wind, but of course leaving the swell unaffected.

Engineers would desire to possess at least so much knowledge of the properties of matter in the several forms of gases, liquids, and solids, as would enable them to form some picture in their minds, imaginary though it may be, of the nature of the phenomena encountered in their daily practice. Twenty years ago, Crookes suspended in an exhausted vessel by a torsion fibre a light beam, having at one end a blackened pith disk of 2 square inches' area, and found that the pressure on the disk due to the radiation of a candle 6 inches off, was $\frac{1}{800}$ of a grain. This balance, and still more the radiometer ever revolving in an exhausted globe under the gentle stimulus of light, or as Tesla showed sixteen years later in an unexhausted globe, under the more powerful stimulus of a high-frequency electric current, have rendered the hypothesis that the pressure of a gas is due to molecular bombardment, at least conceivable by an engineer, but to picture in the mind the motion of the molecules of a liquid, and still more of a solid, is by no means so easy.

The evaporation of moisture is so familiar a phenomenon, that we regard it without wonder, and ask for no explanation. Yet the change from visible water into invisible vapour is difficult to realize, except upon the hypothesis that the molecules are in motion, and that under the influence of heat and other agencies the range of the path of certain molecules is extended beyond the sphere of mutual attraction, and so they fly off from their companions. Crookes has aptly remarked that at molecular distances the boundary surface of a basin of water in an open room will not be a plane, but turbulent like a stormy ocean, and he has shown that negative electrification adds energy to the upper stratum of molecules, and makes them bound off faster. Similarly the evaporation of a solid, like ice, is difficult to realize except upon the same hypothesis. At freezing-point, ice-vapour has a pressure of about $\frac{1}{11}$ lb. per square inch, and the pressure-curve meets the water-vapour curve at only a slight angle. The bombardment of molecules is conceivable here, as it is in the case of the electrical evaporation of gold or silver in a high vacuum, where, although the mass remains cool, the infinitesimally small surface-molecules fly off at high speed and become deposited on the interior of the glass vessel, converting the latter into a mirror. However difficult it may be for engineers to see in their mind's eye molecules of all the substances they deal with more or less in motion, although apparently at rest, it is at least as difficult to regard a substance

like glass as porous, and yet under the stimulus of electricity the sodium in a sodium amalgam on one side of a sheet of glass will pass through into the pure mercury on the other side without affecting the weight or transparency of the glass, whereas when potassium is used instead of sodium the molecules cannot get through the interstices in the glass. Again, it is difficult for a practical engineer to conceive that under any circumstances a pull of 10 tons per square inch would shorten the magnetic extension of a bar $\frac{1}{200{,}000}$ of the length; and yet experiments have shown that it is only necessary for the bar to be tested in a magnetic field of certain intensity for that result to follow. Truly, as Lord Kelvin remarked, "deeper questions are always present" in every branch of an engineer's work, and it avails him little in many things to rely solely upon what is popularly known as "plain common sense," based upon his own general experience.

An engineer is, indeed, daily brought face to face with the fundamental mysteries of the universe. He cannot calculate what speed a railway train would acquire in a given time without an appeal to the same Newtonian laws of motion which affect the remotest star, nor can he estimate the true efficiency of a steam-engine without reference to the mysterious $-461°$, the zero of absolute temperature. Although pre-historic man demonstrated ages ago, by the frictional ignition of dry wood, that work can be converted into heat, the reverse operation, the conversion of heat into mechanical power, was not thoroughly appreciated even half a century ago. It is the latter conversion, however, which chiefly interests the engineer; for although hot bearings and other unpleasant experiences constantly remind him of the possibility of converting mechanical energy into heat, he regards it rather as a nuisance than a blessing. And yet, if we look deeper into the question, we find that after all it is the conversion of energy into heat which accounts for the present existence of the earth itself, and all upon it. It is to the energy liberated by the contraction of the sun's diameter that the world is indebted for heat, since physicists have demonstrated that if the sun had been a solid lump of coal it would have burnt out millions of years ago, and Langley has calculated that all the coal in the Pennsylvania coal-field, though it would last the United States 1,000 years, would only supply the sun with heat for one-thousandth of a second. Every square foot of the sun's surface represents some 8,000 HP., and the whole surface no less than 500,000 millions of millions of millions of horse-power. Indirectly, the engineer utilises a minute fraction of the sun's energy, as the rainfall works his waterwheels and

turbines, atmospheric currents his windmills, and wood is used as fuel for his steam-engines.

Long before the coal-fields of the world are exhausted, there is little doubt the workers on the borderland of engineering will have discovered some plan of tapping the inconceivably great stores of energy around us. The very earth we live on is whirling around like a huge fly-wheel, and if we could only find some way of utilising its momentum we could draw upon it for ages for all the power we want without appreciably affecting the speed of its revolution or the length of our day. It is, indeed, naturally drawn upon now in various things intimately associated with the work of the harbour and dock engineer. The flow of the tide in enormous volume up and down a river is accompanied by a vast expenditure of power in overcoming the frictional resistance of the river-bed, in the grinding of shingle into sand, and in the transport of sandbanks from one part of the river to another. Even the flow of the water through the sluices of locks involves a loss of energy, as does the working of a tide-mill, which latter is one way of utilising, as the others are of destroying, some of the earth's momentum. No true engineer will believe that with so many sources of energy around us the progress of mankind and the work of the engineer will cease with the exhaustion of our coal-fields.

It has been prophesied by many high authorities that in the future all discoveries of great moment will prove to be the outcome of exact measurements. Engineers have always attached great importance to accurate measurements, and are constantly introducing words in their specifications insisting upon the same, although they well know that exactitude is unattainable, and that the vagueness of the meaning of the provisions for accuracy in contract documents often leads to costly litigation. It would be well for the latter reason alone, apart from other considerations, if the Institution of Civil Engineers could define authoritatively what interpretation should be given to such words as "accurate," or "perfectly true to dimensions," which, in practice, must necessarily vary in meaning according to the class of work to which they refer. If, for example, a qualified engineer were asked what constituted a "correct" survey, he would reply by another question —What is the survey for? If it be to produce a plan from which measurements are to be subsequently taken by scale, it would be obviously useless to adopt refinements of observations in the survey which would be beyond the power of the draughtsman to record on the plan, and a "correct" survey in that instance would mean

one in which the error did not exceed, say, $\frac{1}{2,000}$ of the length, a degree of accuracy attainable by chaining. If, on the other hand, the object of the survey were to obtain data for the calculation of the exact spans of such a structure as the Forth Bridge, a very different interpretation would be given to the word "correct," and the mode of procedure would be wholly different. In the case referred to, the first action of the engineers was to recover from the Ordnance Survey Department the original trigonometrical stations in the neighbourhood and the calculated lengths of lines, of which General Clarke said it was "unlikely that the error in their lengths would amount to 3 inches in a mile, or about $\frac{1}{20,000}$ of the length, and that it could not exceed 6 inches." The next step was for the engineers to measure their own base line with standard rods and take fresh angles many times over, with the final result that in a length of 4,000 feet the actual difference and presumable error in the Ordnance local survey proved to be 0·2 foot, or $\frac{1}{20,000}$ of length.

For all ordinary engineering purposes, such a degree of accuracy would entitle a survey to be characterised as "correct." In the case of a metallic structure, for example, the deviation would be equivalent to that arising from a change of temperature of but 7°. If, however, the measurement were for the base line of a great trigonometrical survey, a final error of $\frac{1}{20,000}$ would imply inexcusable negligence on the part of the engineers. The Prussian engineers claimed that their measurements of the 3¼-mile base line at Göttingen, and the triangulations connected therewith, were so accurate that the error in the 36-mile long diagonal could not exceed 5½ inches, or, say, $\frac{1}{400,000}$ of the length. To attain this degree of accuracy, compound zinc and iron bars enclosed in double-skinned wooden boxes with water between were used, and the distance between the bars was measured by glass wedges, which were read by eye to $\frac{1}{5,000}$ inch, and by microscope to $\frac{1}{30,000}$. Similarly, in the great trigonometrical survey of India, a most elaborate investigation of the probable errors in length led to the conclusion that they would not exceed $\frac{1}{700,000}$.

The same elastic interpretation of the word "correct" applies to angles, the admissible error in which may range from two minutes to three-tenths of a second, according to the object of the survey.

But although the vagueness of the word "correct," as applied to a survey, occasionally leads to no little difficulty, it is in the carrying out of works that expressions of the kind constitute an ever-present cause of differences between the engineer and contractor, and of endless litigation and expense. Ten per cent. and more can

readily be thrown away on the cost of works, if the engineer, either from inexperience or obstinacy, insists upon a reading of such an expression as "exact to dimensions"—reasonable enough in some classes of work, but too strict for the particular class in dispute. It is much to be regretted that some general rules as to limits are not authoritatively laid down for different classes of earthwork, masonry, timber, and steelwork. Of course, in many instances, in machined work a limit of so many thousandths of an inch is specified, or a part is considered "exact to dimensions" if it passes a gauge test. So long ago as 1850 the late Sir Joseph Whitworth exhibited at this Institution a measuring machine for determining minute differences in length. When a standard yard measure, made of steel $\frac{3}{4}$-inch square, was placed in the machine, it was claimed that by means of the micrometer a variation of but one-millionth of an inch could be read. Mechanical measurements of this minuteness are, of course, not required in workshop practice. Probably the nearest approach to such a refinement is in the preliminary operations of "figuring" or polishing the lenses of telescopes. By means of the "spherometer," which is a little instrument with three legs to support it on the glass, and a central micrometer screw to measure the curvature of the lens, it is easy, according to Sir Howard Grubb, to get determinate measures of $\frac{1}{50,000}$ inch, and, by adopting special precautions, even of $\frac{1}{150,000}$ inch, which latter has been found to be practically the limit of mechanical contact. In anything else but a lens this might well be accepted as complying with the specification of "true to dimensions," but in that special case such an error would be quite inadmissible; and indirect tests of much greater refinement, such as infinitesimally increasing the local convexity of the lens by the momentary application of the warm hand and testing the optical consequences of the same, have to be resorted to. Fortunately, as the practical working of many branches of the industrial arts depends for success upon the accurate estimation of quantities much smaller than the preceding, there are often indirect ways of attaining refinements which direct mechanical measurements could not pretend to approach. Thus in the spectroscopic analysis of mere traces of different elements, fractional wave-lengths are read to the $\frac{1}{2,500}$ millionth of an inch. Again, Professor Dewar in his researches on liquid air attained a vacuum of $\frac{1}{2,500}$ millionth of an atmosphere by filling a vessel with mercurial vapour and exposing it to a very low temperature; and Professor Boys, with the simplest possible arrangement of quartz fibre, torsional balance, and mirror, claims to have been able

to just detect an attractive force of the $\frac{1}{20,000}$ millionth of a grain. So much for minute weights and measures; and as regards minute angles, the Darwin pendulum will indicate a movement of $\frac{1}{300}$ second, which would be about the angular measurement of a penny-piece at the distance of 1,000 miles.

It is difficult to realize the minuteness of measurements like the preceding. The smallest gold coin of the realm, if drawn out into a wire $\frac{1}{2,500}$ millionth of an inch in diameter would be long enough to stretch to the sun and back again ten thousand times, and yet the fundamental mystery of the constitution of atoms and molecules would be locked up in every infinitesimal portion of the length of that minute wire. "For the establishment of a truer and more comprehensive theory of elasticity," write the authors of the latest important work on the subject, "we shall probably have to wait until we gain a wider acquaintance with the nature of inter-molecular action." Having reference to the minuteness of the objects of our study, the day may be far distant when engineers will know enough of the nature of the materials they use to justify them in relying upon theoretical investigations alone without verification by actual practical test. Take, for example, the case of a gun of wire construction, the theory of which has been set forth in several elaborate Papers in our Proceedings. It is true, as remarked by Sir Douglas Galton recently, in his Presidential Address to the British Association, that "each successive layer of which the gun is formed receives the exact proportion of tension which should enable all the layers to act in unison"; but, unhappily, experience does not bear out his further remark that "the labours of the physicist have revealed the internal conditions of the materials employed," for the physicist would predict that on proof the gun would undergo no change of form, the calculated stresses all being well within the elastic limit, whereas the proof officer would look for no such stability, but would expect the wire gun, or any other gun, to become slightly oval in bore at places, to decrease in diameter here and increase there, and to lengthen as a whole, or bend in any direction in consequence of the molecular adjustments following the shock of discharge. The days of the practical man, taught in the school of experience, are therefore by no means numbered, for neither the mathematician nor the physicist can aid the manufacturer in cases like the preceding.

When my lifelong friend, teacher, and partner, Sir John Fowler, was President of this Institution, now some thirty years ago, he selected as the chief subject of his Address the Technical Education

of Engineers. Since then so much has been done, that, with the aid of public institutions, training of the kind indicated by Sir John Fowler has been brought within the reach of every working man's son. If anything more remains to be said on the subject, it must therefore be of the nature of a warning, that technical education is of little value unless accompanied by the practical experience, sound judgment, and bold initiative which, rather than book knowledge, characterised the famous members of this Institution in the past. Education will do much, but it will not endow a man with common sense, nor will it make his opinion on a multitude of important subjects worth more than that of any naturally observant person. The northern part of the United Kingdom has always attached great importance to education. In past times it established more schools and burnt more old women as witches than any other part of the kingdom. As early as 1496 an Education Act was passed, providing, under penalty of £20, that "all freeholders put their eldest sons to school for at least eight years to learn Latin and laws," and in 1616 a Privy Council Order provided that "every child be educated in religious and secular learning"; and yet at the very time when Watt, working at Glasgow, was revolutionising the world with his inventions, the Associate Presbytery of Edinburgh actually issued an Address denouncing the repeal of the penal laws against witches as "contrary to the express law of God." On my first visit to the site of the Forth Bridge in 1882, I was told by a fisherman, with obvious local pride, that near that spot the last witch was burnt, and the last Scotchman disposed of as a slave. It appeared that a metal collar, inscribed "Alexander Stewart, perpetual servant to Sir John Areskin of Alloa, 5th December, 1701," had been dredged up in the Forth, and further, that many of the church books and town books showed such entries as "Man and woman burnt for sorcery, Kirk's part, £17 10*s.*; Town part, £17 1*s.*, including £3 6*s.* 8*d.* for coals, 14*s.* for tar barrel, and £8 14*s.* to the executioner for his pains." It is quite clear, therefore, that the superior education of our fellow-countrymen in the North did not endow them with sound judgment; but, on the contrary, their blind acceptance of authoritative statements in books, without due regard to the sufficiency of the data upon which those statements were founded, obviously warped their judgment.

There are not wanting contractors and manufacturers who contend that some of the highly trained young engineers of the present day are not wholly free from faults of the latter kind. It may be well, therefore, for all engineers to remember that most of

their elaborate calculations involve some assumption which is more convenient than true. Thus, although a student may find that all of his authorities assume homogeneity of material, the fact nevertheless remains that such a condition does not exist in practice, and that the calculated stresses based on that hypothesis are not those actually present in the material. The manufacturer knowing this, and knowing also that this want of homogeneity is of vital importance in some cases and of none in others, may not unnaturally complain if an engineer complicates designs to make his work comply strictly with theoretical deductions and with the last decimal place in calculations which are more or less speculative even in the units. Again, for theoretical purposes it is convenient to assume that earthwork is a homogeneous material without cohesion; but the contractor's agent who acted upon any such assumption in forming his gullets or timbering his trenches or tunnels, would soon be seeking another job. It is convenient also to assume uniformity of temperature throughout a given mass of metal, but even a stoker on a steamboat knows from experience that his boiler-plates are stressed quite as severely from the unavoidably inequalities of temperature as from the pressure of steam. Other causes often render purely theoretical deductions uncorrected by practical experience quite misleading. Thus it is frequently impossible in practice to attain the accuracy of workmanship necessary to make the assumed theory operative, and in other cases questions of maintenance are all important. For example, on broad theoretical grounds there would appear to be no doubt that the best continuous brake would be one in which the momentum of the train was utilised to apply the brakes, and the worst type one in which fresh power was generated to destroy that which was already stored up in the moving mass; and yet in practice this conclusion is reversed, and the chain-brake has everywhere been superseded by the vacuum or compressed-air systems.

As so many assumptions are necessarily involved in most theoretical investigations, and so many disturbing influences exist, the young engineer, however much he may have distinguished himself at College in the now indispensable preliminary course of training in mathematics and physics, will do well to mistrust any theoretical conclusion of his own which does not commend itself to the practical man of long experience in that branch of work to which his conclusion applies. Again, under the law an engineer is primarily responsible for the consequences of any accident due to defective design, and there are not wanting indications that the facilities which now exist for scientific and practical training have

made juries inclined to be less lenient than formerly. For many reasons, therefore, the young engineer will do well to be a constant attendant at these meetings, for it was for the purpose of enabling engineers to exchange their practical experiences that the Institution of Civil Engineers was founded.

I am one of those who think that the usefulness of the Institution, great as it has been in the past, may yet be extended. For instance, by the appointment of Committees of Members, other than those on the Council, to report on various questions of interest, not only to engineers, but also to manufacturers and contractors, greater uniformity of practice might prevail, with a resultant saving in the cost of work and an avoidance of subsequent litigation. At the same time much valuable knowledge would be crystallized for the use of members generally, whilst the joint discussion of these Reports at special Council Meetings would bring the leading members of the Institution into closer personal relationship. As regards the work of the Council itself, I have little doubt that, had the wise men who drew up the original Charter of the Institution foreseen that the 156 members then to be legislated for would grow to the present 6,730, they would have provided greater elasticity in the Charter, both as regards the constitution and the election of the Council. Provisions would probably have been made for the representation on the Council of our Indian Empire, the Dominion of Canada, the Australian Colonies, and some of our leading engineering centres in Great Britain and Ireland, for ours is an Imperial Institute and not a local Society; and although such Members of Council might not be able to attend many meetings, they would be a source of strength to the Institution as its recognized representatives elsewhere. This advantage could not be secured without an enlargement of the Council, for with present numbers the work of the various committees could not be satisfactorily accomplished were there many non-resident Members of Council. With an enlarged Council the balloting-list might, of course, include a proper proportion of new names every year, and only the exact number of names in all required for the Council, so that voting members might be spared the present invidious duty of striking out many good names, and at the same time representatives from all parts of the Empire might have a fair chance of election. Any such change would almost necessarily carry with it an alteration of the Charter to enable absent members to record their votes without personal attendance at the ballot. I may, of course, be wrong in thinking that the framers of our original

Charter would have acted thus even if they had foreseen the present state of affairs; but I am certainly right in believing that no one associated with this Institution, be he President or youngest student, would support any change inconsistent with the traditions of the past, nor, on the other hand, would he obstruct any reform which altered circumstances may render expedient in the interests of the members at large.

Sir JOHN FOWLER, Past-President, said he was quite sure the members would appreciate the peculiar gratification he experienced in having, as senior Past-President, to move: "That a cordial vote of thanks be accorded to the President for his Address, and that he be requested to allow it to appear in the Minutes of Proceedings." In the early part of his Address the President had spoken, as he sometimes did, very diffidently of himself, and said that if it had been possible he should have preferred that some distinguished man who was not an engineer should occupy the Chair of the Institution. There could be no doubt that the man to whom he alluded was Lord Kelvin, who unquestionably was one of the most distinguished men the age had ever known; nor could there be any doubt from their experience of Lord Kelvin and his mechanical success, that if he had in early life taken up the profession of an engineer, it would have been a very bad thing for many of them. But, in selecting a Civil Engineer as their President, he desired to say, after an experience of thirty-five years, that they could not possibly have made a better selection than they had done—he meant within the limits of their choice, because the distinguished and experienced Past-Presidents of the Institution were not practically open to them, otherwise he did not know what might have been the result. It was perhaps irregular in a Past-President to express the opinions he had done, but one was disposed sometimes to be a little irregular if there was no objection to it. He was quite sure if all the Past-Presidents could, without irregularity, express an opinion, they would, one and all, agree that the members had made no mistake in their selection of Sir Benjamin Baker. It was not usual on such occasions to refer to the character of the President's Address, and it was not his intention to do so—with one exception. He might perhaps be permitted to refer to the concluding remarks, in which the suggestion had been made, and the hope expressed, that the usefulness of the Institution might be extended, and a suggestion was further offered as to the means by which that extension might be brought about. He most cordially and entirely agreed

with Sir Benjamin Baker in that respect. He would go further and express a fervent hope that fruit might come from the suggestion—that it might not be laid on the shelf—but that action might be taken upon it, for he was sure the time had arrived when action might be advantageously taken, and when the Institution, great as it was, might be immensely extended in its usefulness in the way that had been suggested by the President.

Mr. ABERNETHY, Past-President, in seconding the motion, said the President's Address showed a most comprehensive grasp of engineering. He had no doubt that during Sir Benjamin Baker's presidency the Institution would have a number of valuable Papers and equally valuable discussions. In his long experience he did not remember a Presidential Address dealing so generally with engineering science, mechanical, civil and financial. It was peculiarly gratifying to him that he was able to compliment the Institution on the election of a President who had such a thorough knowledge of all those branches, and he was quite sure that such services would in the future, as in the past, be of great value to the members.

The motion was unanimously adopted.

Sir BENJAMIN BAKER, President, in reply, said he had accepted the office, for reasons which the members could partly gather, without any great enthusiasm, but he could not profess the same feeling of indifference to the very cordial reception which had been given to him, because it led him to hope and believe that he had attained one of the great objects of his life, that of securing the goodwill of his fellow-members.

The President then distributed the Telford, George Stephenson and Watt Medals, the Telford and Manby Premiums, and the Crampton and Miller Prizes awarded by the Council for the Session 1894–95 (Vol. cxxii. pp. 126 and 127).

19 November, 1895.

Sir BENJAMIN BAKER, K.C.M.G., LL.D., F.R.S., President, in the Chair.

(*Paper No. 2873.*)

"The City and South London Railway; with some Remarks upon Subaqueous Tunnelling by Shield and Compressed Air."

By James Henry Greathead, M. Inst. C.E.

Probably the most general requirement of great cities is a good system of internal communication for passengers. This want has been urgent in London for many years; for although the transit facilities outside an area bounded by a line joining the termini of the great railway companies are, with exceptions, fairly good, within that area they are of the slowest and most inconvenient description. Even the suburban facilities are falling behind the requirements of the rapid development of traffic, for there are already large occupied areas, within half-a-dozen miles of the great centres of the east and west of London, which it is impossible to reach in a reasonable time, and with reasonable comfort. It would be interesting to trace the prodigious growth of traffic during the last twenty-five years within the Metropolitan area, brought about by the existing facilities for inter-communication, and the growing requirements of the population. Although this cannot be done now, the Table on the following page, prepared from official sources, will suffice to show that the traffic has increased out of all proportion to the growth of the population.

This statement does not deal with the suburban traffic of the railways entering London, nor with that of the North London Railway, where the facilities have remained practically stationary for a long period. And no account is taken of the development of the cab and private omnibus traffic, which has probably been not less rapid. About three-fourths of this traffic has been carried at a speed of between 5 miles and 6 miles an hour, and its growth

cannot compare with that of the elevated railways of New York where, upon one line alone, about 8 miles long, giving an average speed of about 12 miles an hour, the traffic grew in thirteen years from the opening of the railway in 1878 to over 83,000,000 in the year 1892-93. The passengers carried by the elevated railways and

TABLE SHOWING GROWTH OF TRAFFIC DURING YEARS 1864–1894.

—	Number of Passengers carried.			
	1864.	1874.	1884.	1894.
London General Omnibus Company . .	42,650,000	48,340,000	75,110,000	133,132,000
Metropolitan Railway	11,720,000	44,120,000	75,930,000	88,514,000
District Railway . .	..	20,770,000	38,520,000	42,097,000
Tramways	..	41,930,000	119,260,000	231,522,000
Road Car Company .	..	..	3,060,000	44,610,000
City and South London Railway . .	..	..	..	6,959,000
Totals . . .	54,370,000	155,160,000	311,880,000	546,834,000
Population . . .	2,940,000	3,420,000	4,010,000	4,349,000
Ratio of passengers to population . . .	18 to 1	45 to 1	78 to 1	126 to 1

tramways in New York amounted in 1889 to nearly 400,000,000, or to over 260 times the population of the city. There can be no doubt but that the traffic in London would, with better facilities, have grown more rapidly than it did. Even at the recent rate of increase, if facilities are given, there will before the end of the century be about 200,000,000 more passengers to be carried annually, than at present.

The great want is for facilities for the rapid transport of passengers in the central portions of the metropolis which are now served only by omnibuses, owing to the necessary exclusion of tramways from the congested thoroughfares. It may be stated generally that where the need of communication is most pressing the making of railways in the ordinary manner is most difficult and costly, and is attended with loss and inconvenience to the inhabitants. The ceaseless activity which makes the railways so desirable cannot bear the interference inseparable from their construction.

The question of the relief of street traffic in London has occupied the attention of engineers for half a century and more, and numerous proposals have been made for overhead and underground railways and subways to be worked by various means. In

1867 the late Mr. Peter Barlow, F.R.S., proposed a system of what he called "omnibus subways," consisting of iron tunnels 8 feet in diameter, in which single steel omnibuses, to seat twelve passengers each, were to be propelled by man-power aided by gravity, without stations (in the ordinary sense), the passengers paying in the omnibuses. The stopping-places were to be at one level, and to provide for the differences of the surface level in the more elevated districts, it was proposed to have "three series of subways at different levels, the carriages as well as the passengers being lifted in passing from one to the other." The Tower Subway, referred to later in the Paper, was designed to be worked in this manner. Pneumatic railways were proposed at one time, and a pneumatic tube for parcels between St. Martin's-le-Grand and Holborn was constructed and worked. Railways high in the air, over the tops of the buildings, were from time to time proposed, and schemes for ordinary underground lines in every direction were brought forward—being in several cases authorised by Parliament. It is, however, impossible now to refer more particularly to these indications of the ever-present and growing need for improved facilities. After the completion of the Metropolitan and District railways nothing was accomplished in the way of internal railway facilities in London until the railway forming the main subject of this Paper was constructed.

The Act of Parliament authorising the construction of the City of London and Southwark Subway between King William Street, City, and the Elephant and Castle, Newington, was passed after considerable opposition, in 1884, but it was not until 1886 that the Company, under the chairmanship of Mr. C. G. Mott, was in a position to begin the works, with Mr. Edmund Gabbutt as contractor. In 1887 another Act was obtained for the extension of the line to Stockwell, and a later Act sanctioned a further extension to Clapham Common, the name of the undertaking being changed to that which it now bears. By another Act, passed in 1893, an extension northwards through the City to Islington was authorised.

The first object in starting the works of the original line was to construct the two tunnels under the Thames, because there was great misgiving in the minds of many people as to this part of the work. It was freely predicted that the whole capital of the Company would be insufficient for this portion of the undertaking alone. A temporary stage and shaft having been constructed in the river, immediately behind the Old Swan Pier, near London Bridge, the first tunnel was commenced in October, 1886. The second tunnel was started in March in the following year, and by

June, both tunnels were completed under the river. In July, 1887, work was begun upon the site of the station at St. George's Church, Borough, but it was not until the end of the year that the sinking of the "Elephant and Castle" station shaft was begun; the shaft at the City station being started three months later, in March, 1888. About this time tunnelling was commenced at Kennington Park, on the Stockwell extension.

Before the end of the year 1888, Messrs. Sir W. G. Armstrong, Mitchell & Co., were at work on the hydraulic lifts and machinery for giving access to the stations; and in 1889 the contract for the electrical equipment was let to Messrs. Mather and Platt. The experimental running of the electric locomotives and two of the carriages was commenced in February, 1890, on the City section, and continued from time to time until the completion of the works.

The inauguration by H.R.H. Prince of Wales on the 4th November, 1890, was followed on the 18th December by the opening to the public. In the first half year 174,000 train-miles were run, and 2,412,000 passengers were carried. In the year 1894, 458,000 train-miles were run and 6,900,000 passengers were carried.

Description of the Railway.

Starting under and at right angles to King William Street, near the Monument, at a depth of about 70 feet below the surface, the railway runs under Arthur Street West, and Swan Lane to the River Thames, under which it passes at a maximum depth of 73 feet below high water. The railway continues under Hibernia Wharf and Chambers on the south bank, and the southern approach to London Bridge and below the following streets: High Street, Borough, Blackman Street, Newington Causeway and Butts, Kennington Park Road and Clapham Road to Stockwell where, at the junction of the South Lambeth and Stockwell Roads, the line at present terminates. Except where it passes under the Thames and one property on its south bank, the railway is under the public thoroughfares throughout. The course of the railway is shown in Fig. 1, Plate 1, and it will be observed that two existing railways were passed under, viz. the South Eastern, and the London Chatham and Dover.

The section, Fig. 2, Plate 1, shows the main gradients of the two lines which are, excepting at the termini, carried in two separate tunnels. Fig. 3, Plate 1, illustrates the method adopted for driving the tunnels in loose water-bearing strata (see p. 28). In places it

will be observed that the lines are at different levels, as at Swan Lane, which was not wide enough to admit of the tunnels being placed side by side without encroaching upon private property, Fig. 4, Plate 1, and Fig. 7, Plate 2. At the termini the lines converge into one tunnel in order that the trains may pass from one line to the other. A longitudinal section is given, Fig. 5, Plate 1, of the terminal station at Stockwell, of which a plan is also shown in Fig. 27, Plate 3. The terminal station in the City, Fig. 6, Plate 2, has two platforms for "arrival" and "departure," and a single line of rails; at Stockwell there is a central platform with a line on each side of it, so that two trains may be in the station at one time, to provide for the examination of trains and adjustment of running times, &c. Cross-sections are also shown, Figs. 8, 9 and 10, Plate 2, of three of the intermediate stations. At two of them there is a difference of level of 9 feet 6 inches between the lines; in one case the line further away from the entrance-shafts is the higher, and in the other case the lower. The difference of levels was arranged in order that, with a station upon one side only of the street there should be a minimum number of stairs between the lower lift-landing and the two platforms. Access to and from one of the platforms, Fig. 9, Plate 2, is obtained without any stairs by passengers using the lifts, while the other platform is reached by a single flight of stairs, either up or down, but not both; in the other case, Fig. 10, there are no stairs. At the "Elephant and Castle" Station, Fig. 8, Plate 2, the lines are at one level to admit of a cross-over road and lay-by being introduced between the up- and down-lines, and at this station also access between the lifts and the platforms could be and has been obtained by inclines, and there are no stairs.

The Act prohibited the use of steam locomotives, and the original intention was to use the endless-cable system of haulage. There were to be two cables, one between the City and the "Elephant and Castle," the other between the "Elephant and Castle" and Stockwell, and it was intended in the first instance to drive the former at 10 miles per hour and the latter, the line being straighter and more level, at 12 miles per hour. For this reason the tunnels on the latter section were made somewhat larger than those on the first section, viz. 10 feet 6 inches in diameter instead of 10 feet 2 inches. Owing to the progress made in electric traction during the construction of the line, it was determined to adopt that motive power in preference to the endless cable, which will, however, receive a trial shortly on the Glasgow District Subway.

It was determined first to execute the upper tunnel southwards under the river, Fig. 4, Plate 1, and in October, 1886, the shield for this tunnel was lowered into position to rest upon a platform in the shaft, and the tunnelling was commenced southwards in clay. The work at first proceeded at a very slow rate, not more than 23 feet being accomplished in two weeks. But, as the workmen became more experienced in the use of the novel machinery, and after improvements were made in the appliances, the speed was augmented, until it reached as much as 16 feet per day at a face, and 80 feet per week for many weeks together. This tunnel reached the south bank of the river in February, 1887, and was then driven under Hibernia Wharf and Chambers and near to the southern abutment of London Bridge towards the Borough. The second tunnel was, in the same month, started from the temporary shaft at Old Swan Pier immediately underneath the first, but, taking a slightly different direction, it was brought to the same level as the first at the south bank of the river, which was reached in fourteen weeks from the start, and then carried parallel with it under Hibernia Chambers and the South Eastern Railway.

The upper tunnel was meanwhile driven northwards from the Old Swan shaft, and at about 60 yards from the river the clay was pierced and a large volume of water encountered. The face of the shield was at once closed and a bulkhead and air-lock were subsequently erected in the tunnel. After about 50 yards of the tunnel had been driven under compressed air, the shield again entered the solid clay, and, the joints of the tunnel having been caulked with iron cement, the air-pressure was relieved, and the remainder of the tunnel was driven to the terminus in King William Street under the normal air-pressure. It was observed that the air which escaped from the tunnel found its way out into the river over a considerable length, between Cannon Street and London Bridges; and the pressure of the air in the tunnel automatically adjusted itself to that due to the varying head of water in the river, between high- and low-water.

The upper and lower tunnels were then excavated simultaneously, the upper being about 100 yards in advance. These tunnels were carried round the curve under Arthur Street West, 140 feet in radius, without difficulty; and, one of them having been completed across King William Street to Arthur Street East, the 25-foot shaft was commenced within the building No. 46 King William Street. In order to reduce to a minimum the carting of spoil and materials in the City, a 4-foot square timbered shaft was

sunk within the site of the 25-foot shaft and connected by a small heading with the tunnel below. The whole of the material excavated from the shaft and station was sent down the tunnel to the river shaft, whence it was carried away in lighters.

On the completion of the iron tunnels under the river, land was acquired for the "Borough" and "Elephant and Castle" stations, and operations were commenced at these places by sinking the 25-foot lift shafts, to be used in the construction of the tunnels. When this had been accomplished at each place a heading was driven under the street at right-angles to the course of the railway. Four shields were then lowered and rolled into position for driving the two tunnels northwards and southwards. Meanwhile the company, having obtained an Act for the extension to Stockwell, had let the work to Messrs. Walter Scott & Co. in August, 1887. These works were soon in progress at three points, viz., Kennington, the Oval and Stockwell, and the operations on both sections proceeded simultaneously until the City section was completed.

Temporary Shaft in the River.—For the sake of the ready disposal of the excavated material, and to avoid the delay generally attending the acquisition of property, it was determined to commence the tunnels in the river itself from a temporary shaft sunk into the bed, clear of the foreshore and wharves. Piles were driven into the gravel overlying the clay; and, a working stage having been formed 100 feet long by 35 feet wide, the iron rings of a 13-foot diameter shaft were bolted together and sunk, without pumping, through the gravel and into the clay by means of a grab. To maintain a uniform level between the water in the shaft and that of the river, which rose and fell with the tide about 19 feet, a valve was provided in the shaft lining below low-water level. In this way the material surrounding the shaft was not disturbed by the inflow and outflow of water during the sinking, and the valve was not closed until the shaft was well into the solid clay. The lower portion of the shaft was completed in brickwork in cement with four openings or "eyes" from which to start the two tunnels northwards and southwards, Fig. 4, Plate 1. The temporary shaft was sunk to a total depth of 82 feet below high water; and the lower 9 feet of the shaft were and are used as a sump for the collection of the drainage from the two tunnels, both northwards and southwards. The upper portion above the bed of the river was removed after the length immediately over the tunnels had been closed and made watertight with concrete, asphalt and puddle.

Non-Interference with Street-Traffic.—In 1864, when a joint committee of the two Houses of Parliament considered the large number of railway projects within the metropolis then brought forward, the committee reported against a number of the proposals on the ground that their authorization would involve inconvenience during construction. In applying for the Act for this railway there was much opposition to the granting of power to construct temporary shafts in the streets. It was not urged that the work could be done without them, but that as temporary shafts were necessary, the line should not be made. The power, however, was given, hedged round in some cases by conditions; but it was found after some experience in the new construction that it would not be necessary to sink any temporary shafts, and the power was not exercised. The cost of constructing, maintaining, and filling the temporary shafts was saved, but, on the other hand, the length and cost of underground haulage were greater. The temporary shaft in the river was of great use in constructing the tunnels and the station in the City. Through it were passed all the excavated materials, iron, bricks, &c., not only citywards, but also southwards for a distance of more than ½ mile. From the position this shaft occupied in the river, it offered no obstruction to the navigation, and as all the land-shafts were upon private property of the company there was no obstruction anywhere to traffic.

Separate Tunnels for Up- and Down-Lines.—The advantages of two tunnels instead of one as regards ventilation are dealt with later in the Paper. The other considerations which weighed in favour of separate tunnels may be summarised thus:—

1. They could be constructed where a single double-line tunnel could not be, as for instance, under Swan Lane, Upper Thames Street, Fig. 7, Plate 2. In this locality the value of property is very great, and it is impossible to estimate the saving effected by the adoption of two superposed tunnels here; the lane being little wider than a single-line tunnel.

2. The lines could be placed at different levels at the stations for convenience of access, as at three of the stations on the railway, Figs. 9 and 10, Plate 2.

3. Where junctions are intended or may be required at a future time, the placing of the lines at different levels enables a junction, without a level crossing, to be made without the cost of extended "fly-over lines."

4. A dip could be given to the lines, as carried out on this railway to some extent between the stations, for obtaining a higher speed or lower cost of working or both, maintaining the gradients

against the load, that is to say, approaching the stations, at such a moderate inclination that trains could always surmount them, while giving to the gradients with the load, that is to say, on leaving the stations, such a steepness as to secure rapid acceleration. The approaching gradients are 1 in 100 and the departing gradients 1 in 30. If both lines had been in one tunnel, the gradients with and against the loads would necessarily have been the same.

5. Where headway is important; as, for example, at crossings of sewers, or railways, or under a river-bed, the two tunnels give an advantage as compared with one.

6. Greater safety in construction is secured where the tunnel is of little more than one-fourth the cross-sectional area of a double-line tunnel.

7. The two tunnels are cheaper than one, and involve less carting and disposal of excavated material, a matter of importance in a great city not only to the company but possibly to the local authorities and to the public.

Borings.—Frequent borings, generally 3 inches in diameter, were made from the surface along the course of the railway sufficiently far in advance of the work to allow of arrangements being made to meet the conditions of strata thus ascertained. These borings disclose the interesting fact that the tunnels are throughout their whole length, except for about 50 yards at the City Station, subaqueous. Where they are not under the river, they are under or in water-bearing strata, having generally communication with the river as evidenced by the rise and fall of the water, to a greater or less extent, at intervals of time corresponding with the tidal movements in the river.

Shafts.—The lift shafts, of 25 feet internal diameter, were used for the purpose of constructing the tunnels. They are lined partly with cast-iron segments and partly with brickwork in cement. The iron portion, with a cutting-edge but no external projection, was sunk from the surface through the water-bearing strata and 3 feet or 4 feet into the clay. The tunnel segments were used as kentledge, for which purpose their shape rendered them very convenient. Some details of the cast-iron work are shown in Figs. 24 and 25, Plate 3. Below that level they were continued to the full depth in brickwork in cement, built in sections and in underpinned lengths of about 6 feet, with arched openings for access to both ends of the lifts. A circular passage round about half the circumference at the foot of the shaft was subsequently tunnelled.

By making the joints between the iron segments some time before they reached the water, perfect watertightness was secured. None of the segments were planed or turned, and no internal lining of concrete or brickwork was introduced; the flanges were found to be very convenient for attachment of the lift guides.

In constructing the 15-foot shafts for the stairs, the iron lining was carried the whole depth. The water-bearing strata were pierced in the same manner as in sinking the larger shafts. When the cutting-edge had penetrated 3 feet or 4 feet into the clay, the excavation was carried down as for underpinning in brickwork, but was not intentionally undercut beyond the outside diameter of the iron above. Iron segments, similar to those used in the upper part, were then introduced and bolted to the latter. Through holes in the castings, provided for the purpose, blue lias grouting was then forced by the grouting apparatus, to be described, into the small space behind the castings. This system of construction was found to be very convenient and inexpensive, as avoiding the handling of kentledge and other troubles connected with forcing cylinders down to considerable depths. It was much more expeditious than the underpinning in brickwork, with its accompanying undercutting and greater excavation. In this manner iron-lined shafts of uniform diameter can be carried down to any depth, and in close proximity to buildings. The 25-foot shaft for the City station was sunk within a heavy building, the walls of which were only just far enough apart to contain the shaft, which was carried to a depth of 75 feet through made ground, gravel and clay, partly with iron-lining and partly with brickwork, without injury to the building.

Gradients and Curves.—As has been already stated, it was originally intended to work the traffic by the endless cable system. The gradients and curves adopted were, consequently, steeper and sharper than would have been contemplated for a line to be worked by locomotives. The curves, however, could not have been reduced without taking an altogether different route, involving risk of serious opposition or heavy expense for right of way under buildings, or both.

Dip or Depression between Stations.—On a line with frequent stations and where all trains stop at every station, the provision of a certain dip or depression between the stations, depending upon the maximum speed allowable, is of great advantage to the obtaining of a good average speed and economy of power. Taking the case of a line having stations ½ mile apart, upon which a maximum speed of 25 miles per hour, having regard to

curves, &c., is permissible, to attain this speed as much power must be expended as would lift the train through a height of 21 feet. If the average resistance to movement be taken at 10 lbs. per ton, this would require for the ½ mile between the stations, as much power as would lift the train $\left(2,620 \times \frac{10}{2240}\right)$, or nearly 12 feet. The two together $(21 + 12)$ would require power represented by that necessary to lift the train vertically 33 feet. Thus about two-thirds of the propelling power would be expended in getting up speed to be nearly all dissipated by the brakes in stopping. By arranging a dip of 21 feet between the stations, the same or a greater average speed could be maintained with about one-third to one-half of the power. This principle, which has often been proposed, but which cannot be fully realised in practice, has been carried out where practicable on the City and South London Railway, to the extent of accelerating up to the cable speed originally intended, viz., 12 miles per hour; it is found to be of great advantage in working the line. It would give an additional advantage in working by cable, by reducing the destructive slip between the cable and the gripper while accelerating, thus greatly prolonging the life of the cable.

Station Tunnels.—At each of the stations, for the length of the platforms and at the termini for a greater length, enlarged tunnels were constructed in brickwork. At the termini these tunnels are 26 feet wide and 20 feet high from invert to crown, with walls and arch 3 feet thick. In the City the whole of the bricks used were Staffordshire brindles. At Stockwell the face for 9 inches was brindles, the rest being good stocks. At the intermediate stations there are two tunnels, 20 feet wide and 16 feet high from invert to crown. In the cases where the two tunnels are at different levels, the lower tunnel was constructed in advance of the other, one wall of the upper tunnel being built upon one of the walls of the lower. At the "Elephant and Castle" Station the two 20-foot tunnels were built side by side, Fig. 8, Plate 2, at the same level, in order that a cross-over road, long enough to serve as a lay-by siding for two trains, could be driven between the "up" and "down" lines. Great care was exercised in carrying out the brick tunnels. Heavy timbering was used, and the lengths were short. In the earlier work 5-foot lengths were used, but experience proved that a 9-foot length was better, as enabling the forward face of the excavation and supports for the bars to be in more solid and undisturbed material. Notwithstanding all the precautions taken, however, some slight disturbance of the material

overhead generally took place, but in the later work of Messrs. Walter Scott and Co. this was reduced to a comparatively small amount. All the brick tunnels are in London clay, except a short length at Kennington, where the invert is in wet sand, and where it has not been found practicable to completely exclude the water.

Alignment.—In the absence of intermediate points between the shafts at the respective stations for testing the accuracy of the work, great care was necessary in the alignment. Some exceedingly good results were obtained, for instance, the lines were carried from the 25-foot shaft at Kennington Station, 30 yards off the line of tunnel, to the "Elephant and Castle" Station, a distance of 900 yards, traversing a number of curves, with a total divergence of only $\frac{5}{8}$ inch; and again between the Oval and Kennington with a smaller divergence. These examples are interesting as indicating what can be done with good instruments in careful and competent hands; and credit is due to Mr. Basil Mott and Mr. David Hay for the good results obtained.

Lifts.—As an elementary principle it would seem to be better to bring all stations quite near to the surface, and that, no doubt, was a leading principle when the Metropolitan Underground Railways were laid out; but even on those lines the platforms are often a considerable depth below the street levels, and the ascent and descent of fifty or sixty steps has to be encountered. In 1884, when the proposal was made to raise and lower the passengers by lifts on the City and South London Railway the Mersey Railway lifts had not been constructed, and it was contended by opponents of the project that the lift formed a serious objection. Even since the opening and working of both of these railways, it has been urged against the extensions of the system, both in this country and abroad, that the necessity of employing lifts outweighs all other advantages of the deep-tunnel system. A cursory examination, however, should suffice to show that these contentions are ill-founded. By the use of lifts in London, the construction of the line underneath all sewers and pipes and in the London clay is rendered practicable. The cost of constructing railways near the surface, even if the obstruction of traffic could be permitted, would generally be much greater having regard to the necessity of underpinning buildings, diverting and reconstructing sewers, pipes, &c., and, in narrow thoroughfares, of interference with cellars of houses. The deep tunnels insure against injurious vibration and noise.

The cost of working the lifts is small compared with their convenience, and with the extra cost of and other objections to the

construction of railways near the surface, including in many cases the cost of dual establishments on opposite sides of the street, as at Baker Street and Gower Street on the Metropolitan Railway. The public do not object to lifts, as has been assumed by opponents, but on the contrary would hail with satisfaction their installation at many of the stations on the Metropolitan and other railways in London.

The stations alone might be placed near the surface with dips between them; but this would entail considerable interference with streets and sewers, &c., and the tunnels would be more expensive by reason of their having to be driven for considerable lengths through water-bearing strata under compressed air instead of wholly in the London clay. The railway projected in 1885 between King's Cross and Waterloo embodying this principle was threatened with such serious opposition by the Metropolitan Board of Works and the other local authorities that it was withdrawn.

It was decided on the City and South London Railway to adopt suspended lifts in preference to direct-acting lifts, as being lighter and, for large lifts, more rapid, and, owing to the absence of deep wells, as having every part open to inspection and accessible at all times. It may be interesting to note that this application of the suspended form is a return by Messrs. Sir W. G. Armstrong, Mitchell and Co. to the original lifts of Lord Armstrong, introduced nearly forty years ago.[1] The Author does not contend that suspended lifts would in all cases be the best form; on the contrary he is of opinion that in many cases direct-acting lifts would have preponderating advantages.

There are two lifts in the 25-foot shaft at each station of depths varying between 43 feet at Stockwell and 67 feet at King William Street. The cages are approximately semicircular in plan, and each accommodates between fifty and sixty passengers, who enter and leave at either end. The lifts are worked quite independently of one another. The details of this work are shown in Figs. 24 and 25, Plate 3. The whole of the lifts are worked by pumping engines placed in the engine-room at Stockwell, where the pressure in the main is about 1,200 lbs. per square inch. The pressure and return-water pipes are carried upon brackets placed in the tunnels, Figs. 14 and 15, Plate 2. In addition to the main accumulator at Stockwell, another is placed in the stair-shaft at the "Elephant and Castle" Station for the purpose of equalizing the pressure.

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[1] Minutes of Proceedings Inst. C.E., vol. ix. p. 376.

There are three pairs of pumping engines, so arranged that each can be worked independently of, or jointly with, either of or both the others, and controlled automatically by the accumulator in the usual way.

Permanent Way.—It was thought to be very desirable to avoid the introduction of ballast on this line with a view to the comfort of travellers, having regard to the strong air currents set up by the trains, and to the durability of rolling stock, especially of the electric locomotives, and of the way itself. The rails, weighing 60 lbs. per yard, are placed upon cross sleepers resting directly upon the cast-iron segments of the tunnel, Figs. 14 and 15, Plate 3. The rails, it will be observed, are single-headed and are set some distance from the ends of the sleepers. This permanent way, though subject to the disadvantage of increasing the resonance in the tunnel, gives remarkably good results as regards repairs and maintenance.

Ventilation.—One object in placing the up- and down-lines in separate tunnels was to secure ventilation by the action of the trains. Where trains pass to and fro in the same tunnel their action in renewing the air in the tunnel is comparatively slight, unless frequent passages of short length between the outer air and the tunnel be provided. Any considerable transporting action of the train extends only for a short distance in front of and behind the train, hence the great importance of what have been called the "blow-holes" on the Metropolitan Railways. Where, on the other hand, separate tunnels are provided without an escape for the air, except through the stations and their passages, the draft or blast of air on the approach and departure of a train having a cross-section large in proportion to that of the tunnel, becomes inconvenient and a source of discomfort, while the resistance offered by the air to the movement of the train is considerable. The first objection, but not the last, can be obviated by providing openings to the air of sufficient area at each end of the station platforms. Both can be avoided by placing connecting passages between the tunnels, capable of being enlarged or reduced, for regulating the through draft. This expedient was adopted on the City and South London Railway, and with quite satisfactory results, because in the absence of steam locomotives it is not necessary to renew a large quantity of air on the passage of each train. Sufficient provision must in such cases be made at each station for the successive outflow and inflow of air.

The deep tunnels of the railway give considerable climatic advantages. From observations taken on a hot day in summer and

a cold day in winter, with surface temperatures of 85° F. and 22° F. respectively, the temperatures in the small tunnels were about 60° and 59°, on the station platforms 59° and 50°, and in the first coach of a train 62° and 57° respectively. In foggy weather the atmosphere below is comparatively clear, never exceeding a slight haze, even during dense fog on the surface.

Drainage.—For the permanent drainage of the tunnels, small injector-hydrants were placed in the invert at every depression, and connected to the hydraulic main supplying the lifts and to a 2-inch pipe carried along the tunnel to and up the nearest shaft. These injectors, Fig. 15, Plate 2, have been found most satisfactory in working. They cannot get out of order, and require very little attention. All that is necessary, when water is found to have accumulated, is to open a small valve for a few minutes, and the water is discharged into the nearest sewer.

Depôt.—The connection of the main line with the depôt at Stockwell is by means of an inclined siding, having a gradient of 1 in 3½, Fig. 5, Plate 1, and Fig. 27, Plate 3, up which the trains are hauled by wire rope and a stationary engine. In addition to the plant required for generating the electric and hydraulic power and for compressed air for the brakes, Fig. 28, Plate 3, there is a repairing-shop, a carriage-shed, pumps for water-supply, in addition to that of the Water Company, tanks, stores, &c. Since the opening of the railway, additional sidings have been constructed underground at Stockwell for working the increased train service.

Signals.—The signals, by Messrs. Dutton and Company, are of the usual railway type, modified to meet the special conditions of space. The absolute-block system is employed. Since the opening of the railway intermediate signal-stations have, with the sanction of the Board of Trade, been introduced on some of the longer sections, from which electric signals are given, by the passage of the train, to the station in the rear, enabling the next train to be started without waiting for the train ahead to reach the cabin in advance.

Lighting.—The trains are lighted by electric glow-lamps, but hitherto with not very satisfactory results. Experiments have been tried with accumulators for automatically regulating the pressure. These were, however, found to be unsatisfactory. The lighting by accumulators, charged from the main supply, would be possible, and was originally proposed by Messrs. Mather & Platt, but the necessary cost, weight and space led the directors to postpone their adoption pending the trial of other alternatives, and in the expectation that an efficient regulator would be forthcoming.

There is reason to hope that this desideratum is likely to be attainable in a short time. It was originally intended to light the stations from the main electric current, through accumulators, but this was abandoned on account of the considerable cost and trouble in working, and gas fittings were introduced. As affording a light independent of the running of the generating plant and always available, gas has decided advantages, but electric glow-lamps were soon substituted for it at all the stations, without any distressing variations of light, notwithstanding the absence of accumulators. The gas is now retained for use at such times (e.g. for cleaning the stations at night) as the generating plant is not at work.

Working.—In Table I of the Appendix are given the results of the four complete years' working of the railway since its opening. It will be observed that during that period the train-mileage has increased from 174,435 in the first half year to 230,604 in the last, but the total locomotive expenses have been reduced by some 12 per cent. in the latter. In other words, the locomotive expenses per train-mile have been reduced from 9*d.* to 5·9*d.* In the Paper[1] of Dr. Edward Hopkinson, and the discussion upon it, the electrical equipment and working of the railway have been already described and discussed. The electric locomotives of Messrs. Siemens, Brothers & Co. were referred to, but were not illustrated. The Author has, therefore, included two views of these locomotives, Figs. 22 and 23, and the train is shown in elevation in Fig. 26, Plate III. Up to December 31st, 1894, one of these locomotives had run nearly 76,000 miles. There had been no failures in the armatures, the repairs had been practically nothing, and the wire-gauze brushes had run for eighteen months without changing.

The contract for the first section of the railway was undertaken by Mr. Edmund Gabbutt, of Liverpool, who was unfortunately, through ill-health, prevented from completing it. Messrs. Walter Scott and Co., who carried out the extension to Stockwell and the completion of the City section, were represented on the works by Mr. William Sewell. Mr. Basil Mott, M. Inst. C.E., was the Resident Engineer on the extension, and, after the retirement from the City section of Mr. W. S. McCleary through ill-health, on the whole line. The Author, as Chief Engineer, was fortunate in being able to confer in emergencies with Sir John Fowler and Sir Benjamin Baker, the Consulting Engineers.

[1] Minutes of Proceedings Inst. C.E., vol. cxii. p. 209.

Tunnelling.

Historical.—As long ago as 1818, Sir Isambard Brunel took out a patent for "forming tunnels or drifts underground," the main principle of which was the forming of excavations suitable to tunnels of large dimensions "by an operation nearly similar to that of forming a small drift." The body or shell of the tunnel, he stated, might be made of brickwork or masonry, but he preferred to make it of cast-iron, and to line it afterwards with brickwork or masonry. In his patent specification, two modes of carrying out his system are described and shown, one by means of a number of small cells with friction rollers between them, each forced forward independently by any suitable mechanical aid, but preferably by hydraulic pressure. This method he subsequently employed, without the hydraulic presses, in the construction of the Thames Tunnel. In the other method, which he called a teredo, from its "analogy to the Teredo Navalis," he proposed to work spirally upon a small face of excavation nearly at right angles to the main face of the tunnel. It has never been used, and it would appear to be not quite practicable. The drawings show cylindrical tunnels of cast-iron combined with brickwork or masonry, but in the Thames Tunnel, commenced seven years later, Brunel adopted a rectangular section, probably as being more suitable for his form of shield; though Mr. Henry Law, in his account of the Thames Tunnel,[1] states that "the strata being horizontal and from their proximity to the river, subjected to constantly varying pressure, it was considered that a circular structure would have been exposed to very irregular strains." A circular section would certainly have been impossible of achievement with the form of shield actually employed by Brunel. With the abandonment of the circular section, the idea of using cast-iron for the lining of the tunnel became impracticable.

Though a great engineering triumph with the appliances available at the time of its construction, and a lasting testimony to the genius and spirit of Brunel, the mode of construction of the Thames Tunnel has not been attempted elsewhere; and there can be no doubt that for nearly half a century, that work served as a warning to engineers and capitalists not to embark in any undertaking of a similar character, and no other subaqueous tunnel was constructed. Indeed, so disastrous was that early experience that

[1] "A Memoir of the Thames Tunnel," by Henry Law. Weale's Quarterly Papers on Engineering, 1845, vol. iii., and 1846, vol. v.

in 1868, when, an Act having been obtained for the construction of the subway under the Thames at the Tower, it was desired to let the work, no regular contractor could be found to undertake it. The Thames Tunnel was commenced in 1825 and was finished in 1842.

Tower Subway.—So far as the Author is aware, no other work of the kind was embarked upon until the little tunnel at the Tower, designed by the late Mr. Peter Barlow, F.R.S., was commenced in 1869. In the construction of this cast-iron tunnel, a cylindrical shield was used, which was forced forward as a whole by six screws worked by men inside the shield. The tunnel lining, of 6 feet 7 inches clear internal diameter, is composed of rings 18 inches long, each consisting of three segments and a key-piece, the metal being $\frac{7}{8}$ inch thick and the flanges $2\frac{1}{8}$ inches deep. The shield consisted of a cylinder of a single thickness, $\frac{1}{2}$ inch, of iron plates, made slightly tapered, the larger diameter being at the front end, to reduce the skin-friction of the clay on the outside. At the front end was a cast-iron ring with rounded edge forward, to which was bolted a diaphragm of wrought-iron plates, having a rectangular opening in the middle extending to within a few inches of the top, for the passage of workmen and materials. In rear of the face were fixed the six screws, each $2\frac{1}{2}$ inches in diameter, abutting against the forward end of the completed tunnel by which the shield was propelled. The tunnel is 1,350 feet long and in clay throughout, and with the shafts was constructed within the year 1869; the maximum speed reached being 9 feet per day of twenty-four hours divided into three eight-hour shifts. The shafts, 10 feet in diameter, are respectively about 50 feet and 60 feet deep, and the minimum cover over the tunnel under the river is 22 feet of clay. The shafts and tunnel were carried out by the Author for the company at a cost of about £10,000. Steam-lifts were subsequently placed in the shafts, and a small carriage, holding twelve persons, and of 2 feet 6 inches gauge was hauled to and fro through the tunnel by a wire rope and a 4-HP. steam-engine in each shaft. The number of passengers that could be carried in this manner being too limited to pay working expenses, the machinery was soon discarded, and spiral stairs and a footway substituted, to enable foot passengers to use the subway.

Following the Tower Subway, a short length of experimental tunnel, 8 feet in diameter, was, in 1870, constructed in New York for the "Broadway Pneumatic Railway," and another similar short tunnel was afterwards built in Cincinnati. These tunnels

were not subaqueous, but shields of boiler plate similar to the Tower Subway shield, propelled by small hydraulic presses, were used. A short length of the Cleveland Lake Tunnel was subsequently constructed with a shield 6½ feet in diameter and 6 feet in length, composed of heavy boiler plates. It was propelled at first by means of screws, and afterwards by hydraulic presses. The tunnel lining was of masonry, and was inserted in 16-inch lengths. After 140 feet had been constructed in this way the shield was discarded, it was found impossible to prevent the cracking of the brickwork after each advance of the shield. In the New York and Cincinnati tunnels no attempt appears to have been made to close up the cavities left outside the lining upon the advance of the shield. Tunnelling by shield then fell into disuse in America, so that when in 1872 Mr. E. S. Chesbrough was preparing plans for the proposed tunnel at Detroit, he, after consideration, rejected the shield as unsuitable, and proceeded to construct the tunnel in brickwork in the ordinary way. The Detroit Tunnel was commenced in 1872 and was abandoned in the following year.

Several projects were, however, started in this country for constructing tunnels under rivers by means of shields and by other methods, such as cofferdams and caissons. Acts of Parliament were obtained in some cases, and in others refused; but nothing was actually accomplished until 1886, when the City and South London Railway tunnels were commenced. In one case, however, that of the North and South Woolwich Subway, a contract was let in 1876, and a shield with air-locks, hydraulic segment-lifting apparatus, and other machinery, and a large quantity of the cast-iron segments, were actually constructed to the Author's designs for driving through the sand and gravel forming the bed of the River Thames. The contractors however, owing to difficulties elsewhere, abandoned their contract. The late Mr. T. A. Walker, who did not believe in the shield method, expressed his willingness to carry out the work in his own way, which was to drive the tunnel through the chalk underlying the gravel. In the absence of financial strength, Mr. Walker's offer was accepted by the directors, and he was allowed to proceed with the work; but, having sunk a shaft into the chalk he found it impossible to proceed far with the tunnel, even though compressed air, without a shield, was tried, and the undertaking was subsequently abandoned.

In constructing the City and South London Railway tunnels through loose water-bearing strata, compressed air was in 1887 used in combination with shields.

Lord Cochrane, in 1830, took out a patent for "apparatus for excavating, sinking, and mining," being, to quote his specification, "an apparatus for compressing atmospheric air (into and retaining the air so compressed) within the interior capacity of subterraneous excavations . . . in order that the additional elasticity given to and maintained in the included air by aid of my apparatus . . . may counteract the tendency of superincumbent water to flow by gravitation into such excavations . . . and which apparatus at the same time is adapted to allow workmen to carry out their ordinary operations of excavating, sinking, and mining . . . within the space which is filled with compressed air, and also allow workmen ready passage to and from the space into the open air. . ." In his specification Lord Cochrane describes his apparatus as being an air-lock or locks, a water-column shaft and chain-dredge for materials, to be applied for sinking shafts or driving tunnels. The patent was taken out at the time that the Thames Tunnel was under construction, and the drawing shows a shaft and tunnel, the latter in clay, with air-locks in the tunnel, and an open end under the river. The patent has often been referred to as providing perfectly for the sinking of shafts through loose water-bearing strata, but it makes no provision for tunnelling through such materials beyond the air-lock; and indeed Lord Cochrane does not appear to have contemplated the use of compressed air in tunnels, except in materials impervious, or nearly impervious, to air and water, such as soft clay.

Compressed air was not used in the Thames Tunnel, nor, so far as the Author is aware, in any tunnel for many years. It was used without a shield in the first portion of the Hudson Tunnel, commenced in 1879, where the work was in almost impervious and fairly solid material; and also in a 4-foot 10-inch by 3-foot 10-inch and almost rectangular tunnel composed of cast-iron plates at Antwerp in very fine silty sand in 1879. The Hudson Tunnel was, in 1889, proceeded with under the advice of Sir John Fowler, Sir Benjamin Baker, and the Author, with a shield in combination with compressed air and cast-iron lining.

The first shield of the eighteen used on the City and South London Railway was almost identical in its design and construction with that shown in Figs. 11, 12 and 13, Plate 2, which represent a shield for the 10-foot 6-inch tunnels constructed between the "Elephant and Castle" and Stockwell. It consists of a cylinder 5 feet 11 inches long, of steel plates in two thicknesses of $\frac{1}{4}$ inch each riveted together to break joint with rivets countersunk on both sides. This cylinder was bolted to a strong

ring of cast-iron at the front end, and to this ring were bolted the plates and channel-bars forming the face, and the adjustable steel cutters. The latter were so attached that they could be adjusted to cut out the excavation to the same diameter as, or wider than, the steel cylinder following them; the latter provision being necessary for passing round curves in any direction either horizontal or vertical. In the face was provided a rectangular opening with iron doors upon rollers for sudden closing. It was, however, found in practice almost impossible to maintain these doors in working order, so they were subsequently removed and reliance was placed on timbers cut and kept ready for dropping into the channels placed for the purpose at the sides of the doorway. These were always used when work was suspended at a face, or when wet material was encountered pending the provision of appliances for dealing with such material. The inside of the cylinder in rear of the face was lined with massive cast-iron segments; and to these were bolted, as shown in Fig. 11, Plate 2, six hydraulic presses of $6\frac{1}{2}$ inches diameter. The presses were connected with two hand-pumps, for forcing the shield forward. The same pumps served also to run the rams back into the presses. To the projecting ends of the rams were attached long shoes for carrying the pressure on to the solid part of the cast-iron tunnel-lining without bringing any bending strains upon the rams, or undue pressure on the tunnel-flanges. The rear end of the shield, for a length of 2 feet 8 inches, consisted only of the steel cylinder; and within this the cast-iron segments forming the tunnel-lining were put together.

The tunnels on the first or City section are 10 feet 2 inches in diameter, and were composed of rings 1 foot 7 inches long, each ring consisting of six segments and a key-piece, Fig. 14, Plate 2. Southwards of the "Elephant and Castle," they are 10 feet 6 inches clear diameter, Fig. 15, Plate 2, in rings, 1 foot 8 inches long. The flanges of the tunnel are $3\frac{1}{2}$ inches deep and $1\frac{3}{16}$ inch thick, and the plates are nearly 1 inch thick on the City section; on the Extension the flanges are $3\frac{5}{8}$ inches deep and plates $\frac{7}{8}$ inch to $\frac{15}{16}$ inch thick. All holes were cast in the plates and flanges, and in no case was there any tooling of any kind upon the plates. They were cast from soft grey pig and dipped into a composition of pitch and tar while hot, which formed a good tenacious glazed coating upon them when cold.

The joints are shown in Figs. 16, Plate 2, and were found to be satisfactory. In the horizontal joints were placed, at the time of erection, soft-pine packings $\frac{1}{4}$ inch thick; and in the

vertical joints a rope of tarred hemp between the bolts and the "chipping edge." Subsequently the whole of the joints were packed or pointed with Medina cement. Where, however, the tunnels were driven through water-bearing strata, iron cement was caulked into the joints in place of the Medina filling, and with excellent results, for the tunnels in these positions are absolutely watertight. These caulked joints were made before the compressed air was taken off.

The shields were at first not made to do any of the excavation beyond the shearing off by the adjustable cutters of a thin slice of material round the circumference; but subsequently in driving through clay, the Author introduced a series of wedges or piles in front of the face. These were fixed in position against the front of the shield, and were made to enter the solid clay about 2 feet in advance of the cutting-edge by the hydraulic pressure driving the shield. The effect was to expedite the work and reduce its cost materially, the speed being practically doubled. The wedges were free to pass by the nodules of septaria, common in the London clay, without unduly straining the shield or presses. The timbers of the small heading, driven about 6 feet in advance of the shield, were, for a length corresponding to the advance of the shield, previously slackened to allow movement of the material inside the circle of wedges to take place towards the heading.

In the second half of 1888, 2¼ miles of the tunnels were driven, or an average of nearly 2,000 feet per month, or about 80 feet per day, at an average of six working faces. Frequently 100 feet per day were accomplished; and for long periods the tunnels in clay were carried forward 13 feet 6 inches at each face per day of twenty-four hours, divided into two shifts. It is worthy of remark that the men, who were miners and labourers from railways, sewers and similar works, showed remarkable readiness in adapting themselves to work so different from any to which they had been accustomed. Starting with a conviction that the new system was inferior to that to which they had been accustomed, and not hesitating to express their opinions, they soon came to see that the innovation had merits of its own, and eventually that it was superior to the old method. Once satisfied on this score they threw themselves into the work; and men whose lives had hitherto been spent in filling and running "muck," were to be found bending their energies to the working and guiding of the shields, erecting the iron, performing the grouting operations, and making the joints, with method and celerity. During the

whole progress of the works there was no fatal accident, speaking well for the forethought of the contractors and the carefulness of the men.

The shield should be very strong at its front end; and, unless in fluid or semi-fluid material, the tail end need not be very stiff. In all cases where the diaphragm forming the face has been placed well forward, and as near as possible to the cutting-edge, which has been made very stiff, no trouble has arisen. Any change of shape, however slight, at the cutting-edge must, as the shield progresses, tend to increase, and will inevitably lead to trouble. By increasing the strength at the front end, and reducing the shell or cylindrical plates of the tail-end to the minimum, consistent with safety, the annular space left by the advance of the shield round the outside of the tunnel-lining is reduced, and the quantity of grouting correspondingly diminished. This shell has been generally made in two or more thicknesses of steel plates, with rivets countersunk on both sides, thus giving a smooth surface both inside and out, without projecting cover-plates.

The doorway in the face of the shield should be placed as low as possible, in order that where the use of compressed air becomes necessary the portion of the face of the shield above the opening may form a safety-screen in the event of a sudden inflow of water, accompanied or not by material, from the outside. The water could not of course rise above the lower edge of this screen so long as the pressure of the air is maintained. The advantage of a screen in this position is that it is always at the front. The joint between the shield and the tunnel is always good enough to prevent great escape of air, and can by proper grouting be kept almost air-tight, even while the shield is being moved.

Segment-lifting.—The Author designed in 1873 for the Woolwich Subway a hydraulic segment-lifting apparatus which was made and on trial found to work admirably. The segments of the South London tunnels, however, weigh only 4½ cwt. each; and it was found that six men could easily and quickly place the six segments in their respective positions, using for the upper two and for the key-piece a light temporary stage, necessary in any event for bolting together the rings and segments. Small pulley-blocks were found useful for slinging the lower side segments, but beyond these no other mechanical appliances were employed. The objections to the employment of any lifting appliance in a small tunnel are that it interferes with other operations, it does not save time, and is somewhat costly to make and maintain. In

large tunnels, however, some such apparatus is essential, the segments being too heavy to handle and having to be lifted considerable heights.

Hydraulic Presses in the Shield.—The hydraulic presses in the shields used in the City and South London Railway were supplied with water from two cisterns placed inside the shield by hand-pumps one on each side of the platform in the shield, Fig. 11, Plate 2. These hand-pumps generally forced the shield forward in about ten minutes, overcoming the skin friction and the resistance due to wedging and cutting the clay in the face. The pressure varied between 500 lbs. and 1,800 lbs. per square inch, depending upon the number of presses in use, the projection of the cutters and whether the tunnel was being driven in a straight line or on a curve. A reversing valve enabled the rams to be driven back by the same pumps either singly, in groups, or all together.

In the case of large shields having a great number of presses, and requiring a considerable volume of high-pressure water, or where the pressure required to advance the shield is very great, it is expedient to set up a pumping-plant on the surface for the propulsion of the shield. This involves the fixing of high-pressure pipes between the surface and each shield with sliding or flexible connections at the shield. To avoid this, electric motors placed in the shield may be employed, especially when electric-lighting and haulage are used in the tunnel; or a small compressed-air engine may be used, deriving its supply of air from that used for grouting and ventilation. Where the work is proceeding under compressed air the air-engine may simply have its exhaust carried back through the bulkhead; though as a rule the latter arrangement would require a large engine because of the comparatively low pressure, 20 lbs. to 30 lbs. per square inch, thus available for working it. In long tunnels up to 16 feet or 17 feet in diameter it would, however, be difficult to improve upon the simplicity, handiness, and small cost of the hand-pumps in the shield. Very little time would be gained by the use of mechanical power, though the men would be saved some fatigue.

Grouting by Compressed Air.—In the construction of the Tower Subway, grouting was employed to fill the cavity left by the advance of the shield. This was accomplished by a hand syringe, the lime being mixed with water in a tub. The result was not satisfactory because the grout had to be sufficiently fluid to flow into the syringe and was too fluid for good work, and the pressure that could be applied by the syringe was not sufficient to force it properly home into the spaces to be filled; moreover, this

method of working could only be employed upon a very small scale. The Author, some years later, devised the grouting apparatus first used in the City and South London tunnels, Fig. 17, Plate 2. A cylindrical vessel, capable of withstanding a pressure of 70 lbs. or 80 lbs. per square inch, has through its axis a shaft or spindle working in a stuffing-box at each end of the vessel, and provided at one or each end with a handle outside, and carrying, inside the vessel, a number of paddles. The lime and water are introduced through an opening at the top, having a lid capable of being closed air-tight; and the mixture is discharged by compressed air through a length of flexible hose-pipe ending in a branch and nozzle, the nozzle being inserted in holes in the tunnel lining provided for the purpose. The smaller grouting pans are usually worked by two men; one continually keeps the paddles revolving and opens and closes the air- and discharge-valves, while the other has charge of the branch at the end of the hose. As the space is gradually filled, the holes through which the grouting is discharged are successively closed. Beginning at the lowest hole, grout is forced in until it reaches the hole above it; the lower hole is then plugged and the nozzle applied to the higher, and so on until finally the highest hole, in the key-piece, is reached, and the full pressure is brought upon the grout.

After experiments with Portland and Medina cements and blue lias lime, the Author came to the conclusion that the last was in some respects preferable; and, as it was much cheaper than the cements, he adopted it for the City and South London Railway tunnels. Portland cement has, however, been used in some cases, and for special purposes Medina has been found to work well. The blue lias lime may be mixed with or without sand, and does not set hard suddenly like cement. It can be mixed with only so much water as it will retain in setting, it adheres to the surface of the iron firmly, and when fresh and used hot it expands in setting. No reliance being placed on the surrounding shell for strength, there is no object in having a shell harder than solid London clay. An admixture of sand has not been generally used with the lime, the extra trouble of mixing and handling the two materials is hardly repaid by the small saving in cost over pure lime. It is very important that there should not be an excess of water with the lime or cement, because shrinkage will follow the throwing off of the excess in setting, which will be greatly retarded and be very uncertain. Medina cement for grouting purposes appears also to be better than Portland cement,

but it has not the quality of cheapness as compared with blue lias lime. At the stations of the railway about 2,000 feet in length of the smaller iron-lined tunnels gave place to the larger brick-lined station tunnels, affording an excellent opportunity of observing the condition of the grouting. It was satisfactory to find that the work was in every way perfect. The tunnels were everywhere encased and every cavity had been filled. In some cases where nodules of septaria had been broken and moved by the cutting-edge of the shield, the lime had penetrated through cracks in the stone and had filled the cavity behind the stone, the lime filling the cracks themselves being sometimes not thicker than a sheet of thick paper.

The compressed-air grouting was found to be a very important factor in the work—not only for preventing movements overhead and deformation of the tunnels, but also for several other purposes. Its uses in connection with tunnelling under compressed air to prevent the escape of the air and for making air-tight locks, and in connection with the sinking of iron-lined shafts, are referred to under those heads. It was also found to be most useful in cases where valuable property such as wine vaults had been disturbed by the construction of the brick-lined tunnels. All that was necessary to make the walls quite solid was to point the cracks with cement; and when the pointing had set, to inject the grouting so as to completely fill the cracks, vents being provided for the escape of the air and for observation. In a similar manner a railway bridge elsewhere, cracked by movements caused by the tipping for an embankment, has been restored and rendered secure at a trifling expense.

The supply of compressed air used for grouting also afforded the means for ventilating the long tunnels during construction. It was found that by allowing the compressed air from time to time to escape when it was not required for grouting operations, by a slight opening of the controlling-valve, not only was good air secured at the face, but the temperature was reduced by the expansion of the air, and the usual large pipes and blowers were rendered unnecessary.

Hauling Underground.—In the earlier parts of the work a timber flooring was laid upon long temporary sleepers, resting at their ends upon the iron lining; and the excavated material was run out, and the iron, &c., brought in, by manual labour. Subsequently the flooring was abandoned, the invert was filled with clay, and the work was accomplished by ponies upon a very unsatisfactory road. Electricity will probably be found to be the best

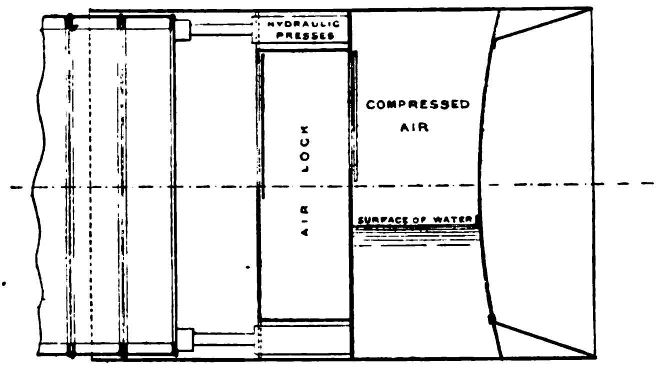
and cheapest means. It has been introduced in the Waterloo and City Railway tunnels, where two small electric locomotives, built by Messrs. Siemens Brothers & Co., do all the traction work.

Tunnelling in loose water-bearing Strata.—At several points on the two sections of the railway compressed air was employed in passing through water-bearing strata. The most notable case was near the south end of the railway at Stockwell, where for a length of about 200 yards the two tunnels were carried through coarse gravel and sand under a head of about 35 feet of water. The longitudinal section of this length is shown enlarged in Fig. 3, Plate 1. For the purpose of this work compressors were erected, and the air was carried a distance of about 300 yards through a 6-inch pipe from them. The tunnels were driven under the normal air-pressure to a point where the cover of clay was reduced to about 5 feet; whence, the air-locks having been erected previously, they were continued under compressed air. It was generally found that the ballast immediately overlying the clay was more open, that is, contained less sand, and that of a coarser character, than in other positions; it was in passing through this very open material that the work was most difficult. The driving of a tunnel is wholly different from the sinking of a vertical shaft under compressed air. In the latter case a uniform pressure of compressed air balances an equal uniform pressure of water; while in the former a practically uniform air-pressure is employed to keep in check a varying pressure of water, the extent of the variation depending upon the height of the face operated upon. For instance, in the Stockwell case, the water-pressure at the top of the tunnel would be that due to a head of about 25 feet; while at the bottom the pressure would be that due to a head of 36½ feet. If the material be close, such as silt or fine sand, there is not much difficulty, provided there be a sufficient cover of material, because the porosity is not so great as to allow of the escape of a large volume of air, while maintaining a pressure sufficient to keep the bottom sufficiently dry. In very coarse sand or, still more, in ballast having but little sand in its composition, it would be impossible to maintain a pressure much higher than that due to the head of water over the top of the tunnel without special appliances and precautions. The difficulty consists in having to work upon, so as to remove, the material from the front of the shield for the whole height of the face, and at the same time to prevent the inflow of a large volume of water, or the escape of an inordinate volume of air. The inflow might involve nothing more than danger to

surrounding structures, where such existed, or it might mean absolute impracticability. In other cases, such as coarse gravel or fissured or very porous rock, it might involve prohibitive expense in pumping. The outflow of air, on the other hand, might, in certain cases, be such as to render tunnelling impracticable, on account of the first cost of plant and the expense of working it. In porous material, therefore, where a large volume of water is to be expected, and the conditions are such as to render pumping impracticable, compressed air is only to be considered if means can be found for preventing its too rapid escape.

The Author many years ago devised means for tunnelling through such water-bearing strata, by working under compressed air with a shield having a face so arranged as to prevent the escape of any large volume of air, *Fig. 18.* The shield was con-

Fig. 18.

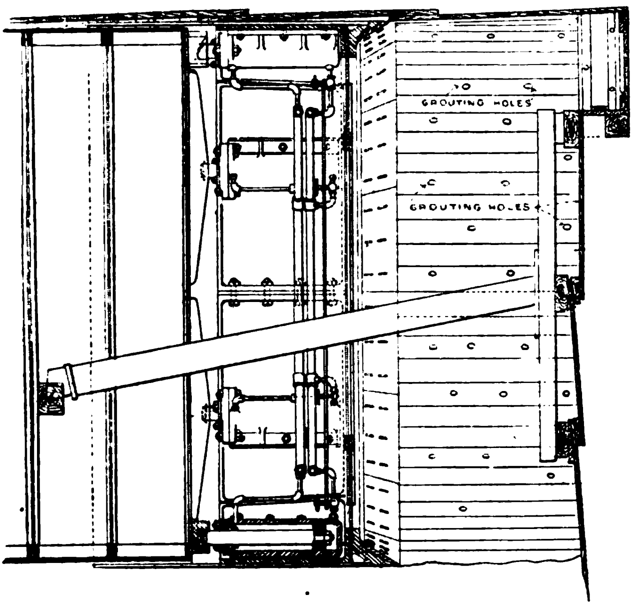


structed for use in the North Woolwich Subway already referred to. It is also practicable, as the Author has proved by experiments upon a small scale, to remove the material from the path of the shield in certain materials by mechanical means, or by a current of water, or by the two combined. In such cases the men might work under a reduced, or even under the normal air-pressure, at depths below that at which they can work at all under pressure. A machine for removing the sand and gravel at Stockwell by mechanical power was constructed and held in readiness for use; but the method first tried was found to work well and was employed throughout.

The shield having been brought to the water-bearing strata, a small heading was driven at the top in advance of the shield, stout poling-boards being used to support the top, resting at one end

upon the forward end of the shield; the heading was then widened out and the polings continued until about three-fourths of the circumference and the whole of the face had been poled, *Fig. 19*. In an ordinary way the polings would not sufficiently prevent the outflow of air, but by frequent injections of lime grout under compressed air, through holes in the polings, as well as through the holes in the iron lining, the escape of air was so reduced that the compressors were not over-taxed. The action of the grout in preventing the escape of air was immediate. The two tunnels

Fig. 19.



Scale, ¼ inch to 1 foot.

were driven in this manner, side by side, under the large mains of the Lambeth and Southwark and Vauxhall Water Companies, supplying a large area of South London, and under sewers and tramways without the slightest disturbance; and this system has since been followed in driving several tunnels under the Clyde and elsewhere in Glasgow through sand, silt, &c. The speed attained under compressed air in the gravel on the City and South London Railway was at each face between 4 feet 6 inches and 5 feet per day of two shifts. The men were found to bear the

compressed air without ill-effect. They were the same men as had worked through the ordinary tunnelling, but the pressure was not more than about 15 lbs. per square inch above the normal.

It is sometimes advisable, and even necessary, to work with an air-pressure below that due to the maximum head of water in the material at the face. For instance, in working in fine sand, by allowing a small inflow of water to take place much below that necessary to carry the sand with it, the pressure of air may be reduced very considerably. This reduced pressure is sometimes of great advantage. The workmen are benefited, and in some cases the work may be more safely carried out, as where there is a comparatively small cover of loose material under a river. In this latter case, a pressure in the tunnel corresponding to that of the head of water at the lowest point of the face, being in excess of that due to the head of water at the highest point of the face, by an amount depending upon the height of the tunnel, or of the portion of the face operated upon, would, in some cases, when the combined pressure of the covering material and the water is less than that of the air in the tunnel, be sufficient to lift or blow up the cover at the face, resulting, probably, in an inrush of water attended with risk to life and other serious consequences; unless other precautions, such as adding weight above or below the material, be taken. It may, also, in more open material, such as coarse gravel, be more economical to pump even the considerable volume of water which would enter with a maximum head equal to the height of the face operated upon, rather than pump the volume of air which would escape through the opening. The average head would be half the height of the opening, and would be independent of the total head of water over the tunnel, the latter being balanced by the air-pressure in the tunnel.

Air-locks.—The first air-lock used was of iron fixed in a bulk-head of brickwork. This was, however, found to be small and inconvenient, and the later air-locks were formed by reducing the size of the iron tunnel by a thick lining of brickwork and concrete, into which two cast-iron door-frames were built, Figs. 20 and 21, Plate 2, leaving a space 12 feet long, 3 feet 9 inches high and 3 feet 9 inches wide for the passage of men and materials. To render this combined bulk-head and lock air-tight, a vertical space of about 3 inches was left in the brickwork at each door-frame, and subsequently filled with Medina cement forced in by the grouting

apparatus under a pressure of about 40 lbs. per square inch. This was found to make an absolutely air-tight barrier. In addition to the main chamber, pipes were built in and through the brickwork of size and length sufficient for passing the temporary rails, pipes, &c., through.

These easily constructed brick air-locks possess the advantage over the iron air-locks first used of mitigating the chilling effect, due to the reduction of pressure, upon the men, hot from their exertions in the warm compressed air, in their egress. The brickwork, absorbing heat when the lock is open to the compressed air, and parting with some of it during the reduction of the pressure when closed against the compressed air, is found to preserve a more equable temperature than the thin plates forming the walls of the iron locks.

When the compressed air is carried a considerable distance through pipes to the bulk-head its temperature may be sufficiently reduced; otherwise measures should be taken to keep down the temperature of the air in which the men work. A spray of water on the outside of the pipes and receiver has been found to answer well.

The workmen employed in the compressed air on the City and South London Railway did not suffer from partial paralysis or "bends." It is true the pressure was not high—about 15 lbs. per square inch—but from observations on this and other works the Author considers that purity or impurity of air has perhaps more effect than pressure upon the health of the men engaged, provided due precautions are taken as to entrance and exit, and the avoidance of chills. It is noticeable that when tunnels have been driven through almost impervious material, as under the Hudson and St. Clair Rivers, and where consequently the quantity of air pumped has been comparatively small, the cases of "bends" were numerous; while in the gravel in London, both at the City and South London Railway and more recently at the Blackwall Tunnel, with a higher pressure, there were in the one no cases at all, and in the other no fatal cases of "bends." Where tunnelling is proceeding in fine sand or in silt, which are almost air-tight, the delivery pipes from the compressors should be extended to the face as the work progresses, in order that the air used in locking may assist the ventilation in the whole tunnel; and provision should be made for a copious supply of air to be delivered at the face. The more highly compressed air employed for grouting is useful for this purpose and for cooling the air, by expansion through a throttled passage, at the same time. In one

instance, at least, this supply has on a serious emergency proved invaluable. In carrying the tunnels of the Glasgow Subway under the Clyde at St. Enoch's a fire occurred, filling the tunnel with suffocating gases and cutting off the men from the air-lock; and but for the air from the hose of the grouting-apparatus the whole gang would have perished. By lying down and receiving the air in their faces the men were able to live during the several hours that it took to reach them by breaking through from the second tunnel.

Iron Tunnels.

Shape of Tunnels.—The circular section will be found to be generally the most suitable for iron-lined tunnels. In a perfect fluid, with the weight of the lining equal to that of the fluid displaced by the tunnel, a circular section, being free from any bending moments, would be theoretically as well as practically the best. In material not fluid enough to flow round the tunnel lining, the circular section is again the best, because, the material surrounding the tunnel affords abutments solid enough to prevent change of shape, ensuring here also absence of bending strains; this applies to all clays as solid as the London clay, and to all gravels and clean sands. For such materials as silt and very soft clays, the circular section would involve bending strains on the lining; but the more nearly fluid the material, the less severe would be the bending strains.

It is convenient to have all the segments of a ring as far as possible alike and interchangeable; and for this reason alone, it is hardly worth while to depart from the circular section for the saving of a comparatively small quantity of excavation. But in soft material a departure to any considerable extent from the circular section, as for instance, the introduction of a flat invert with sharp junction curves between the invert and sides, would generally involve a considerable addition to the weight of the lining to enable it to withstand the unequal pressure.

Cast-iron tunnels possess several advantages over brickwork or masonry tunnels, even where the latter are practicable. They can be made perfectly watertight whatever the pressure of the water surrounding them may be. They can be made stronger than any brick-lined tunnel because, unlike the latter, high pressures do not involve any appreciable enlargement of the outside dimensions of the tunnel; while in the case of a brick lining, after a certain thickness is reached any addition to the section adds but slightly

to the strength. Where excavation is expensive or difficult, the area required for an iron tunnel being materially less than that required for brickwork or masonry, iron tunnels may be constructed in some cases more cheaply, and in all cases with greater safety. As soon as the iron lining is erected the tunnel is practically complete. Iron tunnels are better adapted for construction by shield, and their construction may proceed with much greater rapidity; and thus in large cities there is less interference with traffic by reason of the entire absence or the reduced number of temporary shafts in the streets.

The following Table gives the ratio of the area of excavation to the internal clear area of tunnel in several cases of brick- and iron-lined tunnels—the clear area being taken as 1.

	Ratio of Area of Excavation to Internal Area of Tunnel.
Brick.	
Railway tunnel in clay, 25 feet wide, double line . . .	1·60
„ „ „ 15 feet wide, single line . . .	1·60
Thames Tunnel, two openings, each 14 feet wide . . .	2·22
Iron.	
City and South London Railway, 10 feet 6 inches internal diameter	1·17
Waterloo and City Railway, 12 feet 9 inches internal diameter	1·17
Glasgow Harbour, 16 feet internal diameter	1·16
St. Clair, 19 feet 10 inches internal diameter.	1·17
Hudson (cast-iron), 18 feet internal diameter	1·22
Blackwall, 25 feet inside iron	1·22
„ 24 feet 3 inches inside glazed face	1·30

Combined Iron and Masonry Lining.—Iron alone can be made of the requisite strength and stiffness for the lining of a tunnel of any size. Brickwork or concrete inside the cast-iron should not be relied upon for strength, the two materials being so different in character that they could not be assumed each to take a definite portion of the pressures; and to add internal brickwork for the sake of stiffness is to unnecessarily increase the area of excavation. Any such increase, especially in the case of large subaqueous tunnels, is to be avoided as adding to the difficulty and cost of the work. It is generally desirable, however, to introduce a lining of concrete between the internal flanges of the iron, and perhaps a little beyond to give a smooth internal face, the whole of the iron being thus embedded in lime or cement inside and out. The smooth internal face is also desirable in the smaller railway tunnels as being less noisy and offering less resistance to the flow of air than the unlined iron with projecting flanges.

Since the tunnels of the City and South London Railway were

constructed, a number of other cylindrical iron-lined tunnels have been similarly executed of greater and smaller diameters, for various purposes, in England and abroad. In Table II of the Appendix is given a list of these tunnels. Several small tunnels for gas-supply and drainage purposes, the smallest 4 feet in diameter, are not included in the list.

The Paper is accompanied by numerous tracings, from which Plates 1, 2 and 3, and the *Figs.* in the text have been prepared.

APPENDIX.

Table I.—City and South London Railway.

Date. Half-year ending	Number of Passengers.	Receipts.	Working Expenses.	Train-Miles.	Locomotive Expenses.	Locomotive Expenses per Train-Mile.
		£	£		£	d.
June 30, 1891 . .	2,412,343	19,638	15,521	174,435	6,522	9·0
December 31, 1891	2,862,105	20,244	15,516	188,666	6,099	7·7
June 30, 1892 . .	2,885,262	21,520	15,098	188,944	5,843	7·4
December 31, 1892	3,318,752	22,653	15,390	214,417	6,200	6·9
June 30, 1893 . .	3,251,306	23,159	14,964	217,664	5,754	6·3
December 31, 1893	3,215,151	22,821	14,762	224,101	5,693	6·1
June 30, 1894 . .	3,504,954	24,295	14,990	227,363	5,770	6·1
December 31, 1894	3,454,499	24,253	14,762	230,604	5,672	5·9

Table II.

Date of Commencement.	Tunnels.	Number of Tunnels.	Internal Diameter of Iron.		Length of Single Tunnel driven by Shield.	Strata.	Maximum Depth below Water-Level where Subaqueous.	Length of Ring of Iron Lining.
			Ft.	In.	Yards.		Feet.	Inches.
1886	City and South London, Thames, &c. (Electric Railway)	2	10 10	2 6	11,200	Clay, sand and gravel	75·0	19 20
1889	St. Clair River (Grand Trunk Railway)[1] .	1	19	10	2,000	Clay, hard and soft	78·0	18½
1889	Blackton Reservoir (Stockton and Middlesbrough Waterworks)[2]	1	13	6	142	Shale. .	..	21
1889	Hudson River (railway)[3]	1	18	0	600	Silt, soft .	100·0	20
1890	Fidlers Ferry, Mersey (Liverpool Water)[4]	1	9	0	270	Alluvial beds, clay, silt, &c. .	52·0	18
1891	Kingston, Thames (Southwark and Vauxhall Water)[2]	1	8	4	180	London clay	..	20
1891	Glasgow Harbour, Clyde (roadways and footway)	3	16	0	720	Boulder clay, sand, sand and gravel .	62·0	18
1892	Blackwall, Thames (roadways and footways)[5]	1	25	0	754	Clay, silt, sand and gravel .	80·0	30
1892	Glasgow District Subway, Clyde, &c. (Cable Railway)[5]	2	11	0	6,500	Clay, silt, sand	70·0	18
1893	Mound, Edinburgh (North British Railway)[6]	2	17	6	250	Made ground	..	18
1893	Clichy-Asnières, Seine (drainage siphon) .	1	7	7	500	..	58·0	about 20
1894	Waterloo and City Railway, Thames, &c. (Electric Railway)[5]	2	12 12 23	2 9 0	5,000	Clay, sand and gravel	62·0	20

[1] Heading for masonry tunnel commenced 1886, abandoned 1887. [2] No compressed air used.
[3] Commenced 1879 without shield; 1889 with shield. Work not completed.
[4] Commenced 1888 without shield; 1890 with shield.
[5] Work not completed. [6] Not subaqueous. Compressed air used.

Discussion.

Sir Benjamin Baker.

Sir BENJAMIN BAKER, K.C.M.G., President, said the members were indebted to the Author for a very interesting Paper, which he had no doubt would be followed by an equally interesting discussion. As a Member of Council the Author was disqualified for receiving a medal or premium, and the only thing that the members could award him was a vote of thanks, which he felt sure they would give with all sincerity.

Mr. Mott.

Mr. C. G. MOTT remarked that a year or two after the City and South London Railway Companies' Act had been obtained he had been approached with the suggestion that he should take the Chair of the Company and endeavour to get the work carried out. After the experience obtained in connection with the Thames Tunnel he had not been hopeful that the work would be successfully accomplished; and it was not until he had visited the Tower Subway that he had been satisfied that the work was practicable. It had shown him at once that the principle which the Author had proposed had been previously adopted with success. He had then felt that the carrying out of the work was a practical measure under the able control of the Author, aided by Sir Benjamin Baker and Sir John Fowler. It had been his fortune to be connected as a director with the companies that had carried out during the last twenty years three great tunnelling operations in England—the tunnel under the Severn, carried out by Sir John Hawkshaw; the tunnel under the Mersey, by Sir Douglas Fox and Mr. Francis Fox; and the present tunnels, by the Author. He had been struck by the wonderful power exhibited by the leading engineers of the country in meeting all the difficulties arising in the course of construction. In the case of the Severn Tunnel the influx of what amounted to a river of water had been attended with the greatest difficulty, but it had been overcome, and the tunnel had been worked for years, and was now one of the essential parts of the Great Western Railway. The Mersey Railway, if not financially successful, was carrying a large traffic between Liverpool and Birkenhead. In regard to the City and South London Railway, the many points of difficulty which had arisen had been overcome by the ability of the Author and the extreme thoughtfulness he had bestowed upon every question of the construction.

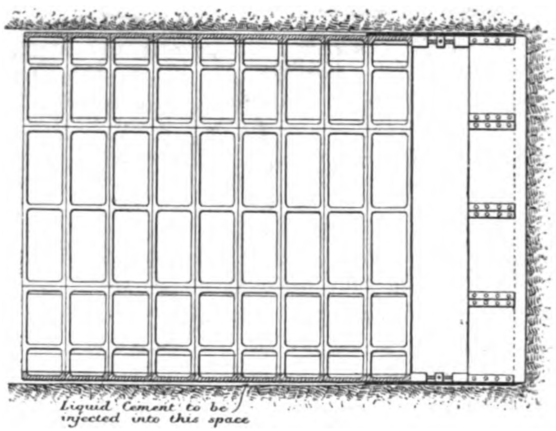
The only method of crossing rivers used by our ancestors had been by boats or rafts. Then had come the discovery of bridges, and so greatly had they been valued in the middle ages that it had been regarded almost as a religious act of devotion to construct one. For centuries men had constructed bridges and had gradually improved them till the time of Telford and his Menai Suspension Bridge, and finally the great Forth Bridge had been constructed. Bridges might now be considered to have arrived at the climax of their construction, and it was now a question, considering the increased use made of rivers, whether it was not still more desirable to pass under them by means of tunnels. That method had been successfully tried by Brunel, but only with the greatest difficulty and at the greatest cost. In the case of the Severn and the Mersey the system had been adopted at great cost, but in the case of the Thames the cost was marvellously small, for he did not think that the amount expended on both of the tunnels of the City and South London Railway under the river exceeded £30,000. In view of the difference in cost by adopting the simple method which the Author had employed, great results might be expected from it in the future. The question had been often raised of constructing tunnels under the sea, such as the proposed Channel Tunnel, and that between England and Ireland. To the latter he thought there could be no possible objection, and he hoped that it would be before long carried out. The difficulty that had arisen was in the working. In a Paper[1] by Mr. Bateman and Mr. J. J. Rèvy the whole question of the Channel Tunnel had been discussed, and a proposal had been made for working it by atmospheric pressure by means of the tide. That appeared to him an ineffective and cumbrous mode of working, although it was very ingenious and worthy of the great engineer who proposed it. He thought that the question had now been solved by the introduction of electricity. There was no difficulty in running an electric train through any tunnel of that length, and he thought it was time that some steps were taken to try a long tunnel of that description between England and Ireland. Mr. Mott.

Mr. Crawford Barlow rose with a view of adding a few facts which had come to his knowledge, from being in possession of the papers of his uncle, the late Mr. Peter William Barlow. In 1863, when Mr. Barlow was sinking the cylinders of Lambeth Bridge, Mr. Barlow.

[1] Report of the British Association for the Advancement of Science, 1869, pp. 206–209.

Mr. Barlow. the idea occurred to him of propelling cylinders horizontally for the purpose of tunnelling. Accordingly, in 1864, he took out a patent for constructing tunnels, where they were to pass under rivers, or under towns and places where the upper surface could not without serious injury be broken up or interfered with, by means of a cylinder of somewhat larger diameter than the external diameter of the intended tunnel, made preferably of wrought iron or steel. The forward edge of this cylinder was made comparatively thin, and the earth was continuously removed from within it. The cylinder was from time to time forced forward a short distance to admit of a ring of iron being put together within its inner end, of a strength suitable for

Fig. 29.

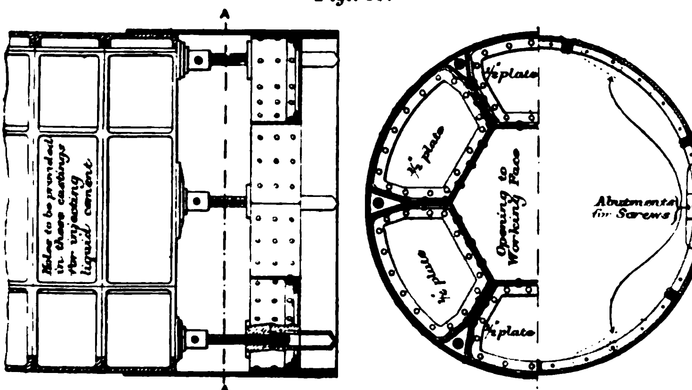


Scale, ¼ inch to 1 foot.

forming a permanent lining to the tunnel. It was desirable that the thickness of the iron of the cylinder should be as small as possible, in order that the space between the outer surfaces of the rings and the earth which surrounds them might not produce subsidence in the surface of the land above. In *Fig. 29* was shown a longitudinal elevation, partly in section of a portion of a tunnel composed of a succession of rings of iron put together with screw bolts and nuts, and also of the cylinder, by and within which the work of removing the earth was performed. If the soil was weak, provision might be made for the use of poling-boards. The space left between the earth and the exterior of the tunnel might be filled by injecting fluid cement. In 1867 Mr. Barlow wrote the pamphlet to which the Author had referred; and in 1868 he

took out another patent, in which he stated that he formed the cylinder with a transverse partition or end, having through or below its centre an opening, which could be either partially or entirely closed as required. As the cylinder was forced forward through the ground, the earth in front of the end of the cylinder was worked away and taken into the interior of the tunnel through this opening. By this arrangement, should the water at any time break into the tunnel, the upper portion of the interior of the tunnel might at all times be kept supplied with air under pressure, as the closed end would prevent the air from escaping. As soon as the tunnel had been pushed forward sufficiently to stop the leakage of water into the end of the tunnel, the pressure Mr. Barlow.

Figs. 30.



LONGITUDINAL ELEVATION. CROSS SECTION A.B.

Scale, ½ inch to 1 foot.

of air in the tunnel might be relieved. In 1868 the prospectus of the Tower Subway had been brought forward, and in it the report of the engineer referred to arrangements having been made for air-pressure if necessary; but in driving the tunnel it had passed entirely through the clay, and there had been no necessity for the application of this process. The subject was subsequently pursued further by Mr. Barlow, and in 1870 he obtained an Act for the Southwark and City Subway, which was practically the same route as the first part of the present line. A company was formed in 1871, and in the prospectus it was stated that the construction was to be on the same principle as that of the Tower Subway, except that the diameter of the tunnel was to be half as large again—that was the same size as the present City and South London tunnel. The Company, however, could

Mr. Barlow. not be financed, and in 1873 an Act of abandonment was obtained. The present railway had evidently followed on the same lines. The main principles of the two patents of 1864 and 1868, namely, (1) that of the tunnel with cast-iron lining; (2) the wrought-iron or steel cylinder overlapping it; (3) the diaphragm with an aperture which could be closed in case air-pressure was required; (4) screws or hydraulic presses for propelling the cylinder or shield forward; and (5) apertures in the cast-iron lining for injecting the fluid cement in the space outside the lining of the tunnel vacated by the shield, were all included in the design made by Mr. Barlow for the shield at the Tower Subway shown in *Figs. 30*, and it is thus evident that this shield was the progenitor of those used in the City and South London Railway, and, in fact, of all the shields in use in the various subways now being constructed; and he was therefore surprised to find that the Author had not referred at all to the work connected with this mode of tunnelling which Mr. Peter Barlow had done before him. At the same time he congratulated the Author on the wonderful development which had been made in this special mode of subaqueous and sub-metropolitan tunnelling.

Mr. Price-Williams. Mr. R. Price-Williams observed that the apparatus and the method which had been described were destined to have a great field of operation, not only in subaqueous tunnelling, but in many other situations where ordinary tunnelling was impracticable, as, for example, in the case of the construction of subways in the Metropolis to relieve the great congestion of traffic to which the Author had referred. He considered that the time had now arrived when recourse should be had to the admirable system described in the Paper, without which the great and growing congestion would assuredly interfere with the future growth of London, which was not only the metropolis of the kingdom, but the great commercial centre of the world. In an interesting Table the Author had drawn attention to the remarkable growth of traffic, and the contrast which it afforded to the increase of population. The figures were certainly startling, and, as the Author had admitted, he had largely understated, for reasons which he had explained, the enormous amount of passenger traffic in London. He had, for example, entirely neglected the very large traffic on the North London and other railways, besides which there was the great increase in the suburban traffic of the principal railways having their termini in London. It was impossible to say what it would all mean in the course of a few years. In a recent report furnished by a commission on another subject—an aqueous

rather than a subaqueous matter—an estimate had been made of the population of the Metropolis, not the Metropolis described by the Author—the Registrar-General's district—but of "Greater London."[1] He wished to point out the fact that the congestion of traffic to which he had alluded was in a great measure due, paradoxical as it might appear, to the great work of which Sir John Fowler and Sir Benjamin Baker were the engineers—the Metropolitan Railway. It was a remarkable fact that up to the time that the principal main lines of railway connected up with London, the population had continued to congest to such a dangerous degree, that, had it continued longer, it would undoubtedly have seriously affected the health of the inhabitants and prevented the development of the Metropolis; but happily by the construction of the Metropolitan, Metropolitan District and other suburban railways, a relief was found—to use the language of a writer of a leading article in the *Times* on a Paper which he had read before the Statistical Society, on the population of London—"From the time the railways connected up with London, there had commenced an overflow of the population from the congested district which had rendered London safe, and it was really owing to the centrifugal effect of the outflow of the population to the outlying districts of the metropolis that its enormous growth was mainly due." The population of the metropolitan outer area was now nearly six millions, and unless some means were devised, like the admirable method described by the Author for subways in London, its growth would be arrested. He hoped that the attention of the engineering profession would be directed to the large employment of that admirable system for the purpose. He was glad to know that already a commencement had been made in the City and South London Railway. Having had an opportunity of examining the works during their progress, he was glad to congratulate the Author of the Paper on the successful way in which he had surmounted the numerous engineering difficulties he had to contend with; but no one could more fully realise than the President himself—who with Sir John Fowler had been associated with the Author in the works—the energy, the conspicuous ability, the judgment and the resource which the Author of the Paper had displayed in carrying them out. Mr. Price-Williams.

Mr. S. J. WILDE was one of the few who remembered as a boy seeing the working of the old Thames Tunnel, and he hardly Mr. Wilde.

[1] "Report of the Royal Commission appointed to inquire into the Water-Supply of the Metropolis," 1893, p. 11.

Mr. Wilde. thought, in the observations that had been made, sufficient justice had been done to the difficulties under which that work had been constructed. By some mistake it had been carried too near the surface, and a quantity of pure liquid mud had come through, so that it could only be worked by filling up from the surface and tunnelling through—a matter of considerable difficulty. He might mention that the tunnel had been complete in the first instance, and afterwards a dividing wall had been erected quite solid, and at a subsequent time arches had been cut through, which still remained. A solid wall had been made in the first instance to give sufficient strength to the roof. For keeping the tunnel dry, in spite of any weeping through the brickwork, rings of tiles similar to roofing tiles, placed at intervals of about the same width as the tiles, had been affixed to the sides and roof of the tunnel. These were covered with similar tiles placed horizontally, and a series of passages were left, through which moisture could run down into the drains under the centre of the tunnel. All being then covered with cement the tunnel had been rendered perfectly dry.

Mr. Fitzmaurice. Mr. M. Fitzmaurice wished to call attention to the word "subaqueous" as applied to tunnelling. Its meaning was quite clear, but there was a great difference between a tunnel driven through water-bearing strata, and a tunnel with say 20 feet of clay between it and the river. When a tunnel was said to be subaqueous, it was generally supposed to be of a more or less difficult character, although under certain conditions it might be of the simplest possible construction. The majority of tunnels were to a certain extent subaqueous. It had been stated by the Author that the borings taken from time to time disclosed the fact that the City and South London Railway was subaqueous over nearly its whole length, although water difficulties had been encountered only over a very short length. In a reference to the tunnel constructed with a shield at Cincinnati, it was stated that this tunnel was not subaqueous although it went under two canals. It was, therefore, difficult to decide where to draw the line in the use of the word "subaqueous," and he thought that some word should be applied to tunnels which would mean that difficulty had been encountered in connection with water, instead of using the word "subaqueous," which was more or less indefinite. He observed that near King William Street the tunnel passed round a curve of 140 feet radius. It would be interesting to know whether that portion of the line was in London clay, or in water-bearing ballast. Of course, if the excavation could be taken out for some way in

front of the shield, it would be easy to bring it round a comparatively sharp curve; whereas, if the shield had to cut its own way to any extent, there might be a difficulty in bringing it round a much flatter curve. He asked if special castings were used for curves so sharp as that mentioned. The Tower Subway was interesting as being the first tunnel in which cast-iron was used and where the shield was shoved forward as a whole. The subway under Broadway, New York, built by Mr. A. E. Beach, deserved mention as being the first in which hydraulic rams were used to propel the shield forward. The face of this shield was divided into several horizontal floors, so as to break up the natural slope of the material. He thought the Author would have referred to the tunnel between Manhattan, New York, and Long Island, which had been recently completed by Mr. C. M. Jacobs. In this case a shield and cast-iron lining had been used in constructing the portion through soft material, and an air-pressure of 48 lbs. was required for a considerable time. The effect of such a high air-pressure on the workmen was very serious, and some deaths had occurred from this cause. He quite agreed with the Author that a constant change and an ample supply of air was essential to men working in compressed air to keep them in good health. With reference to the patent taken out in 1830 by Lord Cochrane, he could not agree with the Author that the use of compressed air was only contemplated for driving tunnels through materials impervious or nearly impervious to water and air. In the specification of this patent Lord Cochrane stated that in certain cases the air-pressure would have to be equal to the pressure of a column of water of the same height as the surface of the river was above the excavation in the tunnel, thereby implying that the tunnel was in direct communication with the river; and he went on to say, "But it will not be necessary at all times to keep up such a great compression of air in the excavation; only at times when the ground which is in progress of excavation is so loose that there is danger of an irruption of water from the river. When the ground is very firm and safe the degree of condensation of the included air may be greatly reduced." The tunnel started by the Author at Woolwich was, he thought, one of the most interesting things mentioned in the Paper. It was the first tunnel in which compressed air was used, and the arrangements made by the Author for the purpose of driving this tunnel seemed to be very complete. He should like to ask what the size of this tunnel was, and whether the hydraulic erector designed for this work was carried on the shield or was independent of it. The first hydraulic Mr. Fitzmaurice.

Mr. Fitzmaurice. erector had been, he believed, actually used at the Hudson Tunnel, and the first erector carried at the back of the shield was at the St. Clair Tunnel; in the latter case, however, the erector was worked by hand-power by means of suitable gearing. The erectors used at the Blackwall Tunnel were carried at the back of the shield and were worked by hydraulic power. A sketch of the erector proposed for the Woolwich Subway would be of great interest. The Author had stated that cast-iron tunnels could be made stronger than any brick-lined tunnel, because, unlike the latter, high pressures did not involve the enlargement of the outside dimensions of the tunnel. That might be true to a certain limited extent, but if there were high pressures the cast-iron flanges would probably be made much deeper, and therefore it would be necessary to increase the outside diameter. With reference to the comparative merits, as regards cost, of air-locks made of brickwork with door-frames built in, and those made of iron and fixed in a brick bulkhead, he thought any advantage which either might have depended on the length of lock required, and the size of the tunnel. In either case a certain minimum thickness of brick bulkhead was necessary to stand the air-pressure in the tunnel. If the length of lock required was a little less than this minimum a brick lock would be cheaper, but if it was longer an iron lock would probably be cheaper, as if the latter were used the lock might project on each side of the brick bulkhead, whereas with brick lock the brick bulkhead would have to be extended the whole length of the lock and even a little beyond so as to take the door-frames. The cost of this extension of the brick bulkhead would depend on the relative areas of the lock and the tunnel. The advantage of a safety screen in the shield was very great, but he did not think that by means of grouting alone the joint between the cast-iron lining and the shield could be kept tight while moving the shield, and unless this was done the safety screen was inoperative. With reference to the shape of iron tunnels there was no doubt but that the circular was, as a general rule, the best. He did not, however, agree with the Author that in a perfect fluid, with the weight of the lining equal to that of the fluid displaced by the tunnel, a circular section was theoretically free from bending moments. On the contrary, there would always be bending moments under such conditions, more particularly in large tunnels at comparatively small depths below the surface of the water. It was impossible, whatever the surrounding material might be, to eliminate bending stresses, and these must always be of a more or

less uncertain character, as any calculations with regard to them would have to be based on assumptions with regard to the character and natural slope of the surrounding material. Nothing had been said in the Paper with regard to cost. Indeed, it was very difficult to find out the cost of any tunnel work. In the report[1] recently made to the Board of Rapid Transit Railroad Commissioners for the City of New York, he had found prices stated at which several tunnels had been let to contractors in this country; and, as the price for the work with which he was connected, viz., the Blackwall Tunnel, was correct, he assumed the others were also. Among these were: Mr. Fitzmaurice.

	External Diameter.	Per Lineal Foot.
	Feet.	£.
Glasgow District Subway	12	27 for double tunnel.
Glasgow Harbour Tunnel	17	27 for one „
Waterloo and City Railway . .	13	32 for double tunnel.
Blackwall Tunnel	27	125 for one „

The prices were given as for excavation, supplying the iron lining and putting it in place, and the price for the last tunnel referred to that portion under the Thames and for a short distance on either side. From the figures given in the Paper, the Tower Subway, including the shafts on each side, appeared to have cost only £7 10*s.* per lineal foot. The cost of the tunnel constructed under the Seine at Clichy, by Mr. Berlier, which was about 8 feet in external diameter, had been stated as £29 per foot run, including the shafts on each side of the river. In this tunnel an air-pressure of 30 lbs. per square inch had been used, and great difficulties had been experienced owing to the broken and varying nature of the ground.

Mr. Binnie. Mr. ALEX. R. BINNIE thought the members must feel deeply indebted to the Author, inasmuch as the Paper described the first practical attempt on a new line to solve the difficulties of the transfer by locomotion of the vast populations of our great towns. It was distinctly not a theoretical but a practical contribution to the solution of that problem; still further, it was of value as showing within what limits locomotion by such means could be carried out, for there could be no doubt that had the whole of the 3 miles 1 furlong of the City and South London Railway to be constructed under the difficulties which the Author encountered in the gravel bed at Stockwell, although it would probably have been an engineering, it would not have proved a financial success.

[1] "Report to the Board of Rapid Transit Railroad Commissioners in and for the City of New York on Rapid Transit in Foreign Cities," by Wm. Barclay Parsons, Chief Engineer, 1894.

Mr. Binnie. It seemed that all the success of works of that kind depended upon the ability to carry the line or lines through an impervious watertight clay. As soon as gravel saturated by water was reached, difficulties occurred which rendered the construction exceedingly costly—so much so, he feared, that, except for short distances, the expense could hardly be repaid by the ordinary traffic on a long line. In contemplating the construction of such a line as that described, the most difficult—or what might be considered by the uninitiated the most difficult—part was the passage under the River Thames. In the case of the Tower Subway, of the City and South London Railway, and of the line now under construction, the Waterloo and City Railway, those tunnels were matters of comparative ease. They were constructed in the impervious beds of the London clay, and, as he thought it would be admitted, they were as easy to construct as would be a tunnel under Primrose Hill. It was only when that mode of construction approached a bed saturated with water that difficulties really commenced. The advantages of a shield in the case of a railway in clay of that kind were reduced almost to the lowest dimensions. A shield could be worked with a perfectly open face. If a large bed of septaria or nodular concretion occurred in front of the cutting-edge, there was not the slightest difficulty in running out a small heading, or in sending a man ahead of the shield to remove the obstruction. No compressed air was required, and the use of the shield in clay of that kind was confined to the two functions of supporting the roof during the period of inserting the rings and to the facility of pushing it forward and so avoiding timbering. In speaking of shields he was reminded somewhat of the remarks of Sir Benjamin Baker in his Presidential Address,[1] in which he had pointed out that engineers were all more or less dependent—most of them more than less—on the work of their compeers or their comrades, the contractors, and possibly also in some cases very much dependent on the work of their predecessors. He ventured to say that in the early part of the century there existed a mechanical genius, to whose efforts in the direction under consideration engineers were all very largely indebted. He should like to go back to the year 1818, and to take up a patent specification, No. 4202, by Marc Isambard Brunel, with the drawings accompanying it. It was a specification for forming tunnels or drifts underground, and in the drawings there were shown, among many other things, a circular wrought-iron shield

[1] Minutes of Proceedings Inst. C.E., *ante*, p. 3.

proceeding in front of the excavation. Certainly it differed from modern shields in being formed in sections, in cells, as the specification stated. The shield had an iron plate which overlapped the main structure. That main structure, the tunnel, was composed of cast-iron rings, and the whole shield was shown as being projected forward by hydraulic presses. Looking at the shield, he must say that it contained within itself, not the germs only, but all the important features of the shields—of all modern shields in use at Blackwall or elsewhere. The idea was not latent, but was actually described in the specification. Certainly, when Brunel, between the years 1825 and 1842, had to construct the tunnel under the Thames between Wapping and Rotherhithe, he had not adopted that mode, and, with the knowledge of the present day, it could be understood why he did not do so. The mechanical appliances of his time would hardly go to the length of constructing a tunnel which in the specification he pointed out must be, to be of any good, about 20 feet in diameter to accommodate the two lines of traffic. When, however, he had constructed the tunnel he had used a shield. It was indeed a rectangular shield, composed of separate cells with iron plates overlapping the brickwork, and it was thrust forward by screw-jacks, abutting upon brickwork, of which the tunnel was composed; and he ventured to say that it was only those who had to construct a tunnel under a great river like the Thames who could fully appreciate the difficulties, the dangers, the courage and the perseverance which must have been required during that long period of seventeen years in which Brunel was engaged in that wonderful feat of engineering. Passing on to the middle of the period that elapsed between 1825 and 1842, he came to the year 1830, and again he would refer to a patent specification, No. 6018, of a remarkable genius, Admiral Sir Thomas Cochrane (subsequently Earl of Dundonald), who, fully cognisant of the difficulties under which Brunel was working at the Thames Tunnel, took out a specification for an apparatus for excavating, sinking and mining, in which he displayed a drawing of a shaft sunk on the border or margin of the river, and a tunnel projecting out under the river. It was not stated in Sir Thomas Cochrane's specification that it was for the purpose of assisting Brunel, but any one who had read the literature of the times and looked at the specification could not but see that when he uses the words "being a similar undertaking to that which is now executing beneath the river Thames at Rotherhithe," that the two men were working in the same direction. What is found in the specification? A shaft to be sunk by pneumatic pressure, an Mr. Binnie.

Mr. Binnie. air-lock at the top, air-locked at the bottom, a horizontal drift or tunnel running out under the river, also air-locked. He asked any dispassionate person who read that specification to say that Sir Thomas Cochrane had not in his mind the working of the same tunnel that Brunel was engaged upon only under compressed air. He could not trace out that compressed air was used in the construction of any work until the year 1839 at Chalons in France in passing through a bed of quicksand. He had some considerable difficulty in fixing the period at which compressed air was used in driving a horizontal adit or tunnel. As far as he had been able to ascertain, it must have been about the year 1871 or 1872. He had not been able to obtain access to all the papers in the Foreign Transactions, but he believed that about 1870–3 tunnels had been driven in America, not very long ones, under compressed air. In the two patents to which he had referred was seen the beginning of the work they were contemplating. What was involved in the work of compressed air? First it was necessary to determine in some way the greatest depth at which to work. He was now supposing that the work was to be performed in a stratum of gravel or some other material which was saturated with water. He did not think that, with due regard to economy of money and life, it was possible to work at a very much higher pressure than 35 lbs. to the square inch above the atmosphere, that was with a head of water of 80 feet. He should approach the construction of works under a greater pressure than that, only with considerable diffidence. With regard to the upward limit, what was that to be? He was now contemplating carrying a tunnel under a river with a gravelly bed; the bed of that river could not very well be approached with the work of construction within 5 feet or 10 feet. The minimum depth at the Blackwall Tunnel was 5 feet 6 inches, and he did not wish to repeat that experiment. Passing to the two next patents of 1864 and 1868, taken out by the late Peter Barlow, those patents had been most carefully described by Mr. Crawford Barlow. He would make no comparisons, but he would merely ask the members to compare Mr. Barlow's descriptions in his patents with those of Brunel and Cochrane. With a modesty which very well became him, the Author had hardly told the whole history of the subject or stated in what way he was connected with the advance of that mode of construction. The subway at the Tower had been designed about the year 1864 or a little later by Mr. Peter Barlow, who had written a pamphlet, but failed to obtain any reputable contractor who would carry out the work. Although an engineer, the Author, formerly his pupil or

assistant, came forward, and at his own risk undertook the construction of the work as contractor for Mr. Barlow, thereby proving much more effectually than by the taking out of any patent the practicability of that mode of construction. He thought that considerable honour was due to the Author for having come forward at a very critical time to prove the practicability of a mode of construction which had been initiated by Brunel and seconded by Mr. Peter Barlow. That was not the time or the place to say more with regard to the Author of the Paper; they all knew him well, and also knew how much they were indebted to him for many of the improvements that had been adopted in that mode of working. He would ask the members to regard it as an engineering advance, and not as a matter of controversy as to who was or was not the originator. They could all afford, without derogation to their own susceptibilities, to bow in all honour to their predecessors who gave them, many years ago, ideas by which they were now profiting. Turning from the past history of the work to future prospects, he looked with great confidence to this method of work in large towns, which, under proper limitations, he thought would be a fruitful source of further improvement in that great and overpowering problem—how to deal with the subject of moving great masses of population. He was glad to see, from a not very rapid legislature, a suggestion which he thought might aid them in that direction. There had sat in the session of 1892 a joint committee of the two Houses of Parliament on the electric and cable railways of the Metropolis. All who had had to do with communications in the Metropolis, or in any other large town, knew that more than half the expense—much more in some cases—was due to the exorbitant prices demanded for land. In the fourteenth recommendation of the Committee there were some words which might assist them in future:—"As to the terms and conditions upon which the subsoil should be appropriated, the Committee report that, in the case of private property not under public streets, it appears to them to be desirable that companies should be allowed to acquire a way-leave, instead of purchasing the freehold of the land, subject to the terms of the Lands Clauses Act as to compensation." That, in other words, merely repeated what were the present powers of the London County Council for constructing sewers. It could make a sewer anywhere that the public interest required; it need not purchase the fee-simple of the land; it might pay for an easement, and it certainly would have to pay for any damage done to the tenants at the Mr. Binnie

Mr. Binnie. surface. But with railways such as those they were dealing with, carried at such depths as those contemplated by the Author, where vibration was not to be detected and rumbling was not heard, compensation would be small, except where they came across an establishment like that at South Kensington, in which delicate magnetic or electric instruments were concerned. In that case, no doubt, an electrically-worked railway of that description might be utterly subversive of the accuracy which the establishment desired to maintain. But, putting highly scientific research out of the question for all ordinary purposes, railways of that description could, he felt sure, be carried without nuisance or injury under any ordinary private property with perfect safety. There was a detail in the construction about which he did not think there would be any dispute, that was that the Author had designed one of those little adjuncts which added so much to the success of such undertakings, viz. his pneumatic injecting apparatus to fill up the inevitable vacuity which must exist between the lining of the tunnel as the shield went forward. That was one of those little things, taken with electric light and compressed air, which made the modern working of the system so exceedingly easy as compared with what it must have been in times gone by. He felt tempted, although that was not the proper time for the purpose, to dilate more fully on the difficulties of that kind of work. He ventured to hope that at no distant date Messrs. Hay and Fitzmaurice, the resident engineers of the Blackwall Tunnel, would favour the Institution with a Paper on that work; and he thought that when it came to be placed before the members, they would be able much more fully to discuss the difficulties under which that description of work could be carried on.

Mr. Lewis. Mr. W. B. Lewis thought the Author had, in his very modest statements, withheld information which the members would like to possess. A few years ago it had been his duty to look into the construction of the City and South London Railway, with a view to discover what damage it had done to the neighbourhood through which it passed, and questions had been raised upon which a great deal of light had been thrown by subsequent experience. He felt much disappointed that Mr. Binnie had not given some information on that point in connection with the work on which he was engaged. But he desired to ask a question which was of far more interest than the history of the subject. He might say, in passing, that the Author was entitled to much greater honour for adopting other people's ideas, and putting them into practice, than he would be for

recording any specification that might have been laid aside for years before a practical man could be found to apply it. They were greatly indebted to the Author for working out, in a most able and resourceful way, all sorts of expedients for getting over the difficulties of this system. The first thing that had struck him, was that the Author had made a small 6-foot 6-inch passage under the Thames, through clay at the Tower, and he never heard that any property was displaced, or that any damage was done in consequence. The 10-foot 6-inch railway under the Thames near London Bridge had then been constructed by the Author, and some little damage had been caused to the buildings at the Hibernia Wharf. In connection with the same railway, the stations required a much larger tunnel, and considerable damage had resulted. He should be glad to know how far the increase in size accounted for the extra damage. He was afraid that the Author would say that those large tunnels could not be made in the same way, and were not made with a shield. At Blackwall a tunnel 27 feet diameter had been made, and he should like to know whether there had been settlements near any portion of the approaches. Difficulties had occurred—comparatively trivial ones—under the bed of the Thames. To engineers acquainted with tunnelling in clay, it must be apparent that that was the simplest method of overcoming the chief difficulty met with, namely, to cover the clay up speedily after it was opened. In excavating a length of tunnel, before the brickwork could be got in, the clay began to swell, and a great deal of damage was done. In the present case, the clay had been excavated and the lining got in before any such damage could follow. He also wished to ask whether the filling with grouting had been fully tested, and with what effect. He had seen a piece of lining of the grouting taken from one of the tunnels made with a shield. It was a large piece, and looked like a piece of thick plaster. In picking it up he had expected to lift a great weight, but was surprised to find how light it was—so light that it could not have had much density. He wished to know whether that was strong enough to stand the pressure of a great mass of clay. If it had been in a clay tunnel, would the clay compress it in time, and cause movement? The movement was slight compared with that produced after removal of timbers. But he should be glad to know whether the matter had been tested. If the plan described in the Paper was to be considered with a view to general use, it would be governed very much by the cost. It had been spoken of by Mr. Binnie as if its Mr. Lewis.

Mr. Lewis. adoption for any great length of tunnel was out of reason; and he should be glad to know whether that was the case—also whether it would avoid the great pumping necessary with the ordinary practice. In the case of the Blackwall Tunnel the approach at the Deptford side had not been made under pressure, but an immense amount of pumping was necessary, and the whole district was disturbed. The place was not much built over, but some new cottages had been erected by the London County Council, all of which had moved; and a great deal of inconvenience had been felt through the subsidence of the neighbouring soil. Would the system of working with atmospheric pressure do away with some of the pumping, and how would the cost of the atmospheric pressure compare with the cost of pumping, having regard to the damage done in the neighbourhood?

Mr. Binnie. Mr. ALEX. R. BINNIE said that the settlements referred to by the previous speaker had not taken place. The tunnel had passed within a few yards of some large buildings which had been erected thirty years ago by the Blakeley Ordnance Company, and no damage had resulted. He had not said that the work could not be constructed, but merely that it could not be constructed economically if the whole 3 miles were similar to the 200 yards of gravel bed at Stockwell.

Mr. Sewell. Mr. W. SEWELL stated that the drawing of the shield had been placed in his hands by the Author, and he had constructed and put it to work on the undertaking in question. Having used twelve shields during the extended period of the construction of the line, he wished to bear testimony to the fact that very little trouble had arisen with regard to repairs and breakdowns. The only trouble that had occurred in the way of keeping the shields in working order was in the renewal of the leathers in the hydraulic presses, and that, he thought, was chiefly attributable to the grit and dirt getting into the water-tanks from which the pumps were supplied. In a shield constructed a little later under his supervision for the Blackton Reservoir in Yorkshire, the water-tanks had been made with closed tops, with a gauge-glass at the side, as in the case of a steam-boiler, so that the men could see whether there was water in the tanks or not. That prevented the workmen from putting in their tools, &c., and the wearing of the leathers gave no trouble whatever. With regard to the shield, it was perfectly true that when it was put to work in an impervious stratum like London clay there was little difficulty in working it. The process went on regularly, and there was one great advantage connected with it, from a

constructive point of view, which had not been pointed out—only Mr. Sewell. one set of men was required, and they were practically unskilled labourers. He should have no fear in training any ordinary intelligent labourer to take any part in working the shield in a week. The tunnel-bricklayer was thus entirely done away with, and engineers who had anything to do with the construction of a tunnel would know what a great advantage that was. The wages of those men in the country were 1*s.* 6*d.* an hour, and there was great difficulty in keeping up the supply even at that figure. Whatever might be the merits of the shield—and he considered them to be very great—he believed that the grouting apparatus (of which there was no question the Author was the originator) was undoubtedly a very important accessory; he considered that without the grouting apparatus the shield would be practically useless. Unless the cavity formed by the shield in advancing was at once filled by the grout there would be settlement and crippling of the lining, which would soon lead to serious trouble. He thought that the immunity from settlement which had been enjoyed along the line of the iron-lined tunnels was almost altogether due to the grout apparatus. A question had been raised as to the liability of the grouting material to shrink under pressure. When the grout was injected it became, within an hour, quite as hard as the London clay itself, and in a day or two as hard as concrete. That being so he could not see that there was any fear whatever of shrinking arising from the grouting. From the specimens that had been taken out of that portion of the iron tunnels driven as temporary headings through the sites of the stations, there was not the slightest crevice of any kind but what was thoroughly filled, even to the thickness of a sheet of paper, by the grout. In the sinking of the staircase shafts, which took place a little later than that of the main shafts, after they had been carried into the London clay and sunk to a depth beyond which it was not safe to push them, the ground had been cut out, lined with cast iron, and grouted up solid by the grouting pan with great success. The work was carried out quickly and expeditiously, and without the slightest subsidence or crippling of any kind whatever. He should have no hesitation in carrying down a shaft of that kind through almost any strata, to any depth at which compressed air could be used, in that manner. All that was necessary was, in putting in the closing segment, to excavate a little bigger than for the rest, to draw the segment inwards into place and grout the cavity so left with the grouting pan. That being done, the shaft was left perfectly solid, and had given

Mr. Sewell. excellent results. In constructing air-locks for the wet portion of the tunnel, the first lock erected had been built without grouting, and it had been a long time before it could be rendered perfectly air-tight. There was great difficulty in building brickwork which was practically air-tight. In the second lock, a cavity of 3 inches had been left in the fore part of the lock in which pipes were inserted. That was blown full of grout afterwards by the grouting apparatus at a pressure of 40 lbs. per square inch, and the lock was absolutely air-tight, not only at the beginning but during the whole time it was in use. When the tunnel had had to be driven through the bed of sand and gravel (it being impossible to alter the gradients so as to sink below it, and pumping from the surface being out of the question on account of the cost and the great danger to the surrounding property), the great difficulty had been to form some estimate of the amount of air which it would be necessary to provide to make up for the great loss escaping through the working face. At that time there had been no data upon which to proceed with reference to the quantity of air necessary. After some time it had been decided to erect a compressing plant, capable of compressing 1,500 cubic feet of free air per minute. The compressor which had been constructed for driving one tunnel, had two steam-cylinders, 18 inches diameter, working at a boiler pressure of 90 lbs. per square inch, and two air-cylinders 26 inches in diameter, coupled in tandem fashion to the steam-cylinders with 3-foot stroke. Those, when run at a rate of 50 revolutions per minute, gave an actual in-take of free air of 1,660 cubic feet per minute. In working the face by the method shown in the diagrams, during the first six weeks, when the material was of a very open character, it had been found necessary to run the engines at 50 revolutions per minute continuously; in fact, for six weeks the fly-wheel of the engine had never stopped. The anxiety with which the operation had been watched could be readily imagined. But about that time it had been suggested that grout should be inserted behind the poling boards. At first he had great difficulty in getting the men to adopt that method, as they were much prejudiced against it, but it was finally tried, and the effect of the grout behind the poling boards had been marvellous. It had been at once possible to reduce the speed of the compressors to between 30 and 40 revolutions per minute, and to double the progress at the face. He felt quite sure that, had it not been for the assistance given by the grouting apparatus in that way, the tunnel would not have been carried through so quickly, so successfully, or so cheaply as it

had been. The second tunnel had followed exactly on the same lines, the same size of compressor had been used, and the poling treated in the same way. The air-lock had been constructed on the lines followed in the first one, and the second tunnel, from the commencement to the finish, had passed through without the slightest hitch. A great many tunnels had been since constructed; but, as far as he could learn, the same lines had been followed; no great deviation from the mode of working having been adopted. Mr. Sewell.

Mr. E. W. Moir observed that the Author had stated that the circular form of lining was the best adapted to withstand the loads imposed upon a tunnel. No doubt the circular form of lining was best for a shield-driven tunnel, because every shield which had yet been used revolved more or less in its progress forward. In the St. Clair Tunnel the shields had revolved in the same direction, although they had been approaching one another, one 3 feet and the other 5 feet. At the Hudson Tunnel the shield in travelling 2,000 feet had revolved 5 feet. The Blackwall shield had revolved in two directions at different times. There was no accounting for the revolution, but in any other form than a circular one such a revolution would be very difficult to deal with. For that reason the circular form was no doubt the best. With regard to the shape for resisting pressure in the London clay, or in any other strata similar to it, a material that would stand more load as a reaction than the active load it would put upon the lining, it did not much matter what the shape was, for the lining spread out and filled any cavity, getting a reaction at the springing. Soft wood packings had been used by the Author between the ends of his segments; they practically became voussoirs of an arch on pinned ends, and they adapted themselves; if there was any space not absolutely filled by grout, the lining would immediately spread, a reaction was produced and greatly increased the strength. In soft river-mud and such material as was found in deltas, considerable bending moments were produced in the cast-iron lining, tension on the under-side of the roof, and tension at the springing on the back. The bending moments became a serious matter, and the tunnels were much strengthened by the addition of temporary ties across them if sufficient bending resistance was not imparted to the castings. Unfortunately, there did not seem to be any reliable method for calculating the strength of the lining, but there was no doubt that in soft muds the vertical axis ought to be longer than the horizontal one, and the same result might be achieved by partially filling the sides of a tunnel where space was not required with concrete. Inasmuch Mr. Moir.

Mr. Moir. as the cast-iron of such undertakings cost from 30 to 40 per cent. of the total expenditure (in the St. Clair Tunnel he believed it was 33 per cent.), it was a great pity that some means could not be schemed for strengthening the lining, using temporarily a thinner casing, and ultimately reinforcing it by concrete or brick-work. Concrete might be used, and he believed was being used, with great advantage in the lining of the tunnels, and he did not see why, in the London clay and similar strata absolutely water-tight, half or two-thirds of the lining should not be removed, and the lining so removed in short lengths replaced by concrete. An additional key could be inserted in the bottom of the lining, which could be drawn out by a union screw from above, or an hydraulic jack. He was sure a considerable saving would accrue in strata similar to London clay. Of course, in the case of gravel or water-bearing strata it could not be done with advantage. He noticed in the tunnel described, and he believed it was also the case in other tunnels though a shield had been used, the old scheme of getting outside and handling the muck and putting in timbering had been reverted to. That, he thought, was a source of delay and of considerable expense. In the case of the Blackwall Tunnel, and also in the case of the Hudson Tunnel, it would have been impossible to have gone outside the shield and done any mining. The river had broken in twice as it was at Blackwall, and then for days and weeks on end the ballast had been taken out through holes 7 inches by 3, it not being possible to pull back one of the shutters which protected the face. Even then, when the little shutters were opened to draw out the stuff, there was only a streak of fire and the escaping air, mixing the flints in such a hurly-burly as to cause sparks to fly from them. The shield ought to be provided with sufficient force behind it to make it do its own mining instead of getting outside and using it like a cheese-taster, paring off the ragged edges. The shield in New York had never been entered; it was all the time full of Hudson mud that was squeezed through the doors, 2 feet 6 inches by 2 feet, at a speed twelve times the rate of the advancing shield, caused by the great contraction in the area of the openings. A pressure of 1,000 tons had been applied to it, and it was so squeezed out that it could be picked up in great lumps and put on wagons without mining; in consequence only muck-fillers had been needed. It was the same thing at Blackwall. In the clay the face was chopped, and the shield driven at it, as much as 4,800 tons being applied behind it. It was stated by the Author that he preferred brick air-locks to those constructed

with iron cylinders built into a brick diaphragm. He could Mr. Moir. not agree with him for the following reasons. The Author's air-lock, as used at the Subway, was 12 feet long. It entailed a brickwork diaphragm, which was partly composed of blue brick, 16 feet 10 inches in thickness; that was only to withstand a pressure of 15 lbs. per square inch, and on an 11-foot 6-inch tunnel. The brick diaphragm at Blackwall, instead of being 16 feet 10 inches, was only 12 feet 6 inches, and it was in a tunnel 27 feet in diameter, and was designed to withstand a pressure of 35 lbs. to the square inch. Had the system of a brick air-lock been adopted a brick diaphragm 29 feet thick would have been required. The diaphragms at Blackwall were amply strong; they were, in fact, thicker than was needed, because it was necessary to have a certain bearing area for the brickwork on the flanges. He had calculated them on the basis that they concealed a dome within them, which was a very simple way of dealing with the matter. It had been stated by the Author that the opening in the diaphragm of the shield should be placed as low as possible, so that air space should always be left in the roof. No doubt that was an important matter—to leave a space in the roof where a man could always get his head, though he was up to his neck in water. On the 13th of April, 1895, while a tunnel was being driven under the River Yarra near Melbourne, some of the men had gone into the lock and seen Mr. Buchanan, the engineer in charge of the work, making efforts to induce them to open the lock-door. They had seen him waving a signal-lamp to open the door; but before they could do so the water had risen above the bull's-eye through which they were looking. The result had been that Mr. Buchanan and six miners were drowned. The shield which was being used was called in the reports the "Greathead shield"—as all such shields were now called; but generally in America they are called not the "Greathead" but the "Beach" shield. He mentioned the matter because he did not think the provision made in the shield used in the City and South London Tunnel, with a door of that kind, which was the same size as that of the Yarra shield, would be sufficient for safety. He had never found that the space between the tail-end of the shield and the cast-iron could be trusted to be air-tight. At Blackwall two men had been almost continually employed in pugging the place with soft clay. In addition to pugging with soft clay, which made the tail-end of the shield air-tight, and brought into effect the benefit of the hood in the shield, there had been a hanging screen a few hundred feet back from the face, which came half-way down

Mr. Moir. the tunnel. That converted the after part of the tunnel into a great diving bell. Should the water ever rise to its half diameter there was an emergency lock above the level of the bottom edge of the screen through which the men could get—a matter of great importance. On two occasions when the river had risen on them it had flooded the lower locks almost in an instant. The men had had to get in and out through the emergency lock, and indeed they could not have got on without it. Every tunnel of large diameter ought to be provided with a safety screen and an emergency lock as high up in the brick bulkhead as possible. In the Hudson and Blackwall Tunnels shield an attempt had been made to design a kind of submarine boat. Some of the most important features of it had been suggested by Sir Benjamin Baker, who had been consulted as to its design. In reference to compressed air, the Author had said that he had never had any difficulty with the men at the Subway. It had been Mr. Moir's unfortunate experience to see much disaster and death due to compressed air. When he had first gone to New York the men had been dying at the Hudson Tunnel at the rate of one man per month out of forty-five or fifty men employed—a death-rate of about 25 per cent. per annum. The shield had to be erected 2,000 feet out from the banks of the Hudson under a pressure of 35 lbs. per square inch, and that death-rate had increased to seven men in six months. An air-chamber had therefore been made in which the men could be treated homœopathically. It was erected on the top of the shaft, and when a man was overcome or completely paralysed, as he had seen them over and over again, completely unconscious and unable to walk, they were carried into the chamber, where the pure compressed air was raised to about a half or two-thirds of the pressure in which they had been working. The improvement was instantaneous. They were then let out at the rate of about 1 lb. per minute or even less; it took twenty-five or thirty minutes to bring them out, and even in severe cases the men were sent away rejoicing. The death-rate had been so far reduced in that way, that instead of having one man in fifty die per month there had only been two deaths in fifteen months out of one hundred and twenty men. No man ever suffered by going into compressed air unless his eustachian tubes were blocked, which was the mechanical effect of the pressure being on one side of the ear drum and not on the other, producing intense pain, and the man had to go out, unless he was relieved by swallowing or by holding the nose and blowing. He might go on with impunity for six months, and then one day he would come out in exactly

the same way, and find himself paralysed for life. He had known Mr. Moir. a very sad case in which a man worked six months, and was paralysed on his way home, falling between some railway wagons, where he lay all night. The medical lock had no effect upon him, and he remained paralysed to-day, and could not control his organs at all. It appeared to him that when a man went in under air pressure he was in the same condition as a furnace under forced draught. Suddenly three or four times the weight of oxygen was passed over his lung surface, or through his furnace; the system gradually assimilated itself to that increased oxygen, and more combustion went on. When he came out, however, and there was a sudden reduction in the amount of oxygen, the forced draught was shut off, and as it would be in the case of a furnace; there would be a production of carbonic oxide through insufficiency of oxygen to burn up the carbon in the coal. He thought that there was precisely the same effect in the case of compressed air. The carbon went on accumulating in the blood, and the man was actually poisoned by the effects of the carbonic acid or the carbonic oxide. That was proved from the fact that whenever air was of the slightest impurity—much less impure than in the room in which they were assembled, or in the Metropolitan Railway, or in many theatres—increased sickness was the consequence. The air had been analysed in the Blackwall Tunnel every week, and it had been found that if the percentage of carbonic acid was above one part in a thousand there resulted a great increase of sickness. Fortunately there had been no deaths, but there had been one case of paralysis which was cured immediately in the lock, and there had been one or two cases of vertigo, one of which was more or less permanent, though he had heard that the man was now recovering. The great necessity was to have plenty of air. The quantity which was found to produce the required results was 2,000 cubic feet per man per hour. With that quantity a pressure of 35 lbs. per square inch could be maintained without difficulty. No doubt the purity of the air was the great secret of the health of the men.

Mr. J. Wolfe Barry, C.B., Vice-President, had read the Paper Mr. Barry. with great interest. The system of burrowing or dipping so deeply under ground was one which necessitated the employment of means such as had been pointed out by the Author; but he thought from his own experience of London locomotion that the important thing to be considered at the present time was the question of cost, and he hoped that some particulars would be given of the cost of the cast-iron tunnels, so that it might be compared with the cost of the larger tunnels such as had been

Mr. Barry. employed for those very important means of locomotion, the Metropolitan and the Metropolitan District Railways. It had been truly said by the Author that there had been very little increase in the means of railway locomotion in London since those railways were put to work. The real reason for that was that it was now found extremely difficult to make Metropolitan railways pay interest on their cost. The competition on the surface, the improved paving of the streets, combined with the improved road traction by tramways, road-cars, and means of that kind, which would take a passenger up and put him down exactly where he wanted to be placed, rendered it extremely difficult to make Metropolitan railways or any Metropolitan means of communication pay, unless they could be made cheaply. To his mind, therefore, the crucial point for the future of the interesting works described in the Paper was what they cost as a means of locomotion in London. As far as the use of compressed air was concerned, it was perfectly true that it must be of the greatest possible utility in water-bearing strata; but a question lying at the root of the matter was, whether it was advantageous or not to go to great depths, with cast-iron tunnels and compressed air, and in doing so to incur the cost of a very expensive lining. He had recently, in conjunction with his friend Mr. Forman, been engaged in making a railway through the very heart of Glasgow; and he thought it might be of interest to state the cost of the railway, which had been made like the Metropolitan Railway, close to the streets, with large stations and convenient platforms, with few steps and consequently with very easy access. He could give the contract prices through the densest part of Glasgow, along Trongate and Argyle Street, which might be compared almost with the Strand, and certainly with Oxford Street and Regent Street, for traffic. The three contracts worked out thus: In the Bridgeton contract, the contract cost, including everything except station buildings and permanent way, amounted to £94 per lineal yard; the Trongate contract £141; and the Stobcross contract to £90. The contracts had not been adjusted, and therefore he could not say that that would be the ultimate cost; but he knew of nothing that had arisen which would seriously disturb those figures. He had also abstracted some figures from the Trongate contract, which embraced the railway through the most crowded part of the city, with a covered way which was constructed of brickwork arching, but mainly with expensive wrought-iron girders and jack arches. He had taken out the cost, including all the street restoration, the underpinning of property, sewers and drains,

and the laying of the permanent way—in fact, everything but stations and station buildings. In one district the cost was £65 per lineal yard, in another £73, in another £64, in another £113, and in another £115. It would be of interest to compare the figures with the cost of cast-iron tunnels, including the same items of cost as he had included, and not merely the cost per running yard of the tunnels themselves, as it must be obvious that, *cæteris paribus*, the nearer to the street a railway was, the more convenient it must be. It was not an advantage to go down to a great depth into the soil, and the only reason for doing it was the avoidance of cost. He did not think any question of property would disturb the comparisons which he had proposed, because the railways in question went down the middle of wide streets, and very little property had been bought. As far as the working of the underground railways was concerned, it would be clear to every one that there was no reason why deep lines should have the monopoly of electricity. If electricity was the best motive power he saw no reason why it should not be applied to the ordinary underground railways of the country; and he hoped to see it so applied. The French were so applying themselves to the problem. He had seen drawings in Paris of a very powerful locomotive worked by electricity for conveying the ordinary traffic of the railway running from Paris to Mantes. An experimental engine had been running nearly a month, and a larger one was now being built.[1] The engine was not at present strictly an electric engine, except that the wheels were turned round by an electric current. The generator which produced the electric current was on a travelling platform, and the current was produced by an ordinary steam-locomotive boiler, supplying steam to a Parsons engine mounted on the same frame and these engines actuated the dynamos. It seemed at first to be a rather roundabout way of applying electricity, but he had been told by the engineers in France that they were satisfied there was economy in it, and that better results were produced by those steam-engines in the way of economy and fuel, and that the advantage of using electric motors for the revolution of the wheels of the locomotive was very conspicuous in the subdivision of power, the avoidance of reciprocating parts, and matters of that kind. He believed that in two or three months they would hear of one of those engines being at work on the Rouen line. They of course would not

Mr. Barry.

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[1] Minutes of Proceedings Inst. C.E., vol. cxiii. p. 481.

Mr. Barry. assist in the ventilation difficulty of underground lines being in themselves steam-engines working also electrically. He thought that every one must feel interested in the subject of working under compressed air, which had so many aspects in regard to the question of coping with water-bearing stratifications with comparative facility. Although compressed air did not work with the facility that some persons imagined, it enabled them under proper conditions to do many works which had never been before dreamed of.

Mr. Deacon. Mr. G. F. Deacon wished to refer to the tunnel carrying the Vyrnwy Aqueduct (76 miles in length) under the tidal River Mersey. The construction of that tunnel had been a well-marked step in the history of shield work. The undertaking had been singular and remarkable in three respects—firstly, owing to Parliamentary exigencies and a decision of the Board of Trade, the tunnel had been placed where no engineer would have chosen to drive it; secondly, it was the first tunnel ever constructed by means of a shield through entirely water-bearing strata beneath a river tidal or otherwise; thirdly, the difficulties had been at first so great that about nine-tenths of the time occupied in the work only sufficed to complete the shafts and about one-fifth of the length of the tunnel, the remaining four-fifths having been constructed in about one-tenth of the whole period. When it had been found that a tunnel was inevitable, he had at first intended to construct it through the boulder clay formation at a depth of about 80 feet below the level of ordinary spring tides. Without actually requiring the use of a shield, he had stated in the specification for this work his desire that a shield of the kind used in the Tower Subway by the late Mr. Peter Barlow, and subsequently, with certain modifications, by Mr. Greathead for the City and South London Railway, should be employed. He had not believed that in this formation artificial air-pressure need be maintained for the driving, and, having regard to the depth, he had thought it better to dispense with it. The contract had been let in April, 1888. The contractors, however, had not used a shield, but had continued—while tunnelling through boulder clay—the air-pressure employed in sinking the Cheshire shaft. In October, 1889, when the work had reached about 57 feet from the centre of that shaft, the tunnel had been flooded, and in October, 1889, the original contractors had ceased to act. At this period eighteen months had already elapsed, and within three months a contract was let to other contractors, who had elected, and had been permitted, to drive the tunnel at a higher level and above the boulder clay. It had been known from borings which had been

previously made that the strata at this level would be entirely water-bearing, that they would be loose, of constantly varying descriptions, and that they would differ largely even at single sections in the depth of the tunnel. At the point of commencement on the Lancashire shore the invert of this tunnel was 52 feet below high water of ordinary spring tides. The shield used by the contractors was precisely as described by Mr. Peter Barlow, and since used by Mr. Greathead in the London clay of the City and South London tunnel and elsewhere. The first difficulties met with had arisen from the circumstance that the ground had been disturbed by the shaft-sinking operations of the former contract. Cast-iron tunnel segments weighing 4 cwts. each, sawn timber and many other materials drawn down from above had been taken out, and when at length the shield had reached the natural strata they had been found, as expected, to vary—even in the small diameter of 10 feet—from silt, through running sand and small gravel, to rough ballast and occasional veins of clay. It had soon become apparent that the shield was too weak, and after many serious difficulties had occurred the lower part of the cutting-edge had buckled inwards for about a quarter of the circumference to a maximum extent of 15 inches or 16 inches, and at the same time a longitudinal crack had been found near the bottom extending from the tail nearly to the diaphragm. At this juncture the second contractors had objected to continue the work under their contract. Mr. Deacon.

After such a succession of difficulties and in face of the fact that no tunnel had hitherto been completed under similar circumstances, there had been little hope of obtaining contractors to carry out the work for a fixed sum of reasonable amount. Moreover, it had been urged that success was impossible. On the other hand, his observation had led him to believe that the work was not only possible but certain of accomplishment under conditions which might still be ensured. He had previously proposed to the Corporation of Liverpool to carry out the work without entering into an ordinary contract, and nothing more had remained but to do this, or to abandon the work performed, and incur the delay of a further application to Parliament for a new route not only for the river crossing, but for much of the already completed aqueduct. The opposition to his views had been considerable, and there was serious risk of the tunnel being abandoned. In a report dated the 13th of October, 1891, he had expressed his belief thus:—"With a shield properly designed to pass through the different classes of material met with the work can be accomplished by next summer." And he concluded this report as follows:—"The frequent examin-

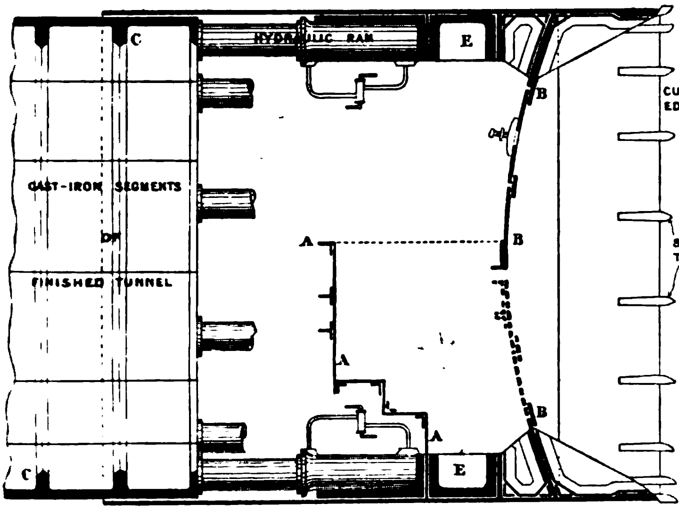
Mr. Deacon. ations of the subaqueous workings, which I have myself made, have only served to confirm my belief in the practicability of the undertaking without immoderate cost. At the same time it should be remembered that I did not seek this method of constructing the crossing. It has been forced, as I venture to think, uselessly upon the Corporation, by the Board of Trade, and it has seriously delayed the completion of the great undertaking of which it forms a small but essential part. Nothing, it appears to me, remains but to push on the work with due deliberation, unslackened energy and full confidence in the result." Wiser counsels had at length prevailed, and it had been resolved to continue the work. He had stated that in his belief the tunnel could be completed "by next summer." It had been in fact completed by the following March.

A new agreement had been entered into with the contractors to continue the work at prime cost under his (Mr. Deacon's) directions as to the methods to be employed, with Mr. Arthur H. Cochrane to take charge of the men and materials; Mr. A. W. Brightmore (who from the beginning had acted ably as Resident Engineer) continuing to act in that capacity. It would have been a long and difficult matter to replace the damaged shield in such a position, and it had therefore been decided to strengthen and repair it, with no certainty, but with a strong hope, that it would survive the future ordeal. Moreover, the ground could not at any other place have been less favourable for the work than that in which the shield then stood. In about seven weeks the operation of strengthening and repairing the shield had been successfully performed. An important addition suggested by Sir Benjamin Baker had been at the same time made. He referred to the bird fountain device shown in *Fig. 31*, which was a section of the shield as used in the construction of four-fifths of the length of the tunnel. When the diaphragm B of an ordinary shield was partially open for access to the face and a "blow" took place, there was nothing to check the momentum and tumultuous energy of the entering water until it had risen in the finished tunnel C above the level of the opening in the diaphragm. A second diaphragm had therefore been introduced at A, a few feet back from the main diaphragm, and having its horizontal sill above the level of any opening—such as that shown by dotted lines—to be used in the main diaphragm. It was clear that—with this arrangement—if the pressure in the tunnel balanced or exceeded the pressure of the water without the tunnel, no serious flooding could occur; while if the second diaphragm at A were absent and the equilibrium was once lost, flooding might continue to a serious

Mr. Deacon.

extent. The device differed entirely from anything shown in the drawings accompanying the Paper; it was in no sense a high-pressure air-lock. With the dotted portion of the main diaphragm removed, a man might stand between the two diaphragms, and even outside the outer diaphragm, with comparative safety, and could always rapidly make his way to the air-lock; and while doing so, the inner diaphragm prevented that calamitous rush of water which had produced disaster in so many cases. A movable lid was provided, as shown by the dotted line at A B. In the case of one blow which had subsequently occurred, it had proved to be practicable to replace this lid

Fig. 31.



Scale, ¼ inch to 1 foot.

and thus further to steady the rush. In another case, however, this could not be done, and a large quantity of water, sand, and silt had been discharged into the tunnel, but happily without loss of life such as might have occurred if it had not been for the modifying effect of this device. At the same time the shield was greatly strengthened by means of a ring of cast-iron girders E and in certain other ways. The steel teeth were also added. The work had been recommenced, slowly at first, but with increased speed as experience had been gained. At the end of six weeks, however, the longitudinal crack already referred to had become worse, and another crack had worked from it circumferentially across the

Mr. Deacon. bottom of the shield. The resistance to progress of the shield had been increasing, and this was found to be due to the fact that the front edge of this fracture had turned down and was ploughing the strata as it advanced. It was impossible to avoid a stoppage. The ground was as bad as it could be; but notwithstanding this and a water-pressure of 54 feet at high tide, a piece of the annular shield was successfully cut out, 6½ feet long and 9 feet wide circumferentially, and replaced with new plates bolted to the cast-iron ring girder, and not connected longitudinally or elsewhere, so that they simply overlapped, were supported by and were dragged over the finished cast-iron work. The portion cut away could not be removed, but was left behind when the shield advanced. The operation had only occupied twelve days, and after this repair no serious difficulty with the shield had occurred. During its progress heavy trunks of oak timber lying prostrate were met with. These greatly retarded the work, and had to be cut away at the rate of 18 inches at a time. The maximum speed attained had been 19 yards a week, and the Cheshire shaft had been reached on the 22nd March, 1892, some months earlier than he had ventured to predict. The whole length, 618 feet, driven with the improved shield, had occupied, including all stoppages, four and a half months. The tunnel was in every sense a complete success. It remained so dry that the hydraulic pumps, worked by pressure of Vyrnwy water—which are automatically started by the rise of water in the sump below the Lancashire shaft—were very rarely brought into use, and then chiefly owing to condensation of moisture on the ironwork.[1] In all successful work of this kind a large proportion of the credit must be attributed to those constantly on the spot, and he could not speak too highly of the care and attention devoted to the matter by Mr. Brightmore and Mr. Arthur Cochrane.[2]

Mr. Greathead. Mr. GREATHEAD in reply said, in reference to the expedient used in the Vyrnwy Aqueduct tunnel for preventing sudden "blows" or inrushes of water and materials through the face of the shield, that the same device had been used in the shield made for the Woolwich Tunnel, *Fig. 18*. A second diaphragm, placed in rear of the face of the shield and extended from the bottom up to a level

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[1] The hydraulic pumps are further referred to in Minutes of Proceedings Inst. C.E., vol. cxv. p. 251.

[2] More complete information on this subject and concerning the principles to be observed in shield tunnelling is given in a Paper by G. F. Deacon, printed in the Report of the British Association for the Advancement of Science, 1892, p. 532.

above that of the top of the opening in the face, made it impossible for the air to escape so long as its pressure did not exceed that of the water, or for the water to enter the tunnel so long as its pressure did not exceed that of the air acting upon the horizontal surface of the water between the face and the diaphragm. The air-lock shown in *Fig. 18* might obviously be placed anywhere in the tunnel; in this case, air-locks had been provided for use in both places, that in the tunnel consisting of two iron diaphragms on wheels, capable of being moved forward from time to time. The account given by Mr. Deacon referred to two tunnels, one of which was started in 1888 without a shield. This tunnel, although compressed air had been used, had been driven in eighteen months, only some 57 feet from one of the shafts, when it was flooded and subsequently abandoned. Soon afterwards, Mr. Greathead had been consulted by Messrs. Cochrane & Sons, who had offered to complete this tunnel. Having regard to the great depth at which the tunnel was proposed to be placed, viz., at one end 113 feet below high water, and to the fact that it was not in impervious strata, he had advised that the offer should be withdrawn, and that it would be much easier to construct a tunnel by shield and compressed air at about half the depth, or some 50 feet below high-water level through the alluvial strata disclosed by the borings. At this time the tunnels of the City and South London Railway had been successfully driven through the most difficult water-bearing gravels, without pumping, as described in the Paper. The second tunnel had been subsequently started by Messrs. Cochrane & Sons, and, as having been responsible for its being placed at the higher level, he would point out that the whole of the difficulties connected with its construction had arisen from the fact that the shield was crippled in some way at the start, probably by contact with the cast-iron segments referred to by Mr. Deacon. The shield was not, as stated by Mr. Deacon, precisely the same as those used in the City and South London Railway tunnels, and described in the Paper. The cutting-edge was made to project about 3 feet in advance of the face of the shield, and it was this projecting cylinder that had become deformed. As stated in the Paper, any change of shape at the cutting-edge would inevitably lead to trouble, and this was not the only tunnel where such trouble had occurred. As soon as this defect was discovered and corrected and the shield strengthened, the work went forward quite satisfactorily. But for this mishap there was no doubt the tunnel would have been driven through in three or four months from its start. Mr. Greathead.

Mr. Greathead. In regard to Mr. Barry's remark as to the cost of work at Glasgow, he would ask whether it would be within the limits of practical engineering to suggest that that which had been done in Glasgow should be attempted in London. He had frequently been in Glasgow, and had seen rows of piles driven down along both kerbs, and he thought that was a mode of construction that could hardly be contemplated in London. Mr. Barry had given the prices of some of the tunnels there, and had made certain deductions from them for the absence of underpinning. He did not think it would be possible to carry out any construction going below the foundations of houses in London without underpinning them. But taking the prices that Mr. Barry had given for tunnels in Glasgow, he could only say that the prices of the two tunnels in the City and South London Railway were about half his minimum. The lowest cost per yard was stated by Mr. Barry to be £90; but the two tunnels of the South London Railway were constructed for £45 per yard, including everything except stations.

Mr. Barry. Mr. Barry said he had not deducted the underpinning, but included it.

Mr. Greathead. Mr. Greathead understood it was included in some cases; but he did not gather that there was any underpinning in the tunnels given as costing £90 per yard.

Mr. Barry. Mr. Barry said that the second prices he had given excluded the stations only, everything else was included.

Mr. Greathead. Mr. Greathead said in the case of iron tunnels there was no street restoration, underpinning of property, interference with sewers and drains, driving of sheet piling in the streets, or surface work of any kind, and the cost per lineal yard of double tunnel, which included the permanent way, was the equivalent, therefore, of the figures given by Mr. Barry, except that it should be borne in mind that the cost of materials, of carting and of labour was higher in London than in Glasgow, and that railways along the Strand, Oxford Street and the City streets would not be constructed "down the middle of wide streets," as Mr. Barry had stated the Glasgow lines were. It would, therefore, have made a more satisfactory comparison if Mr. Barry had given the cost of his underground railway work in London, as, for instance, the tunnel under Cannon Street.

It was not quite obvious that the nearer to the street a railway was, the more convenient it must be. If the railway could be placed quite near, with its platforms not more than 15 feet below the surface, it might be considered preferable to placing it at a depth so great as to involve the use of lifts; but certainly stations

Mr. Greathead.

at 50 feet or 60 feet down with good lift accommodation would be preferable to stations at half that depth without lifts. Then the opposition to railways near the surface, under the streets as well as under houses, would be great on account of apprehended noise and vibration in working, in addition to inconvenience from construction. The avoidance of cost was not, therefore, as Mr. Barry stated, the only reason for going down deep into the soil.

In reference to the shape of tunnels, he maintained his opinion that for an iron tunnel the circular section was preferable to any other under most conditions. Local requirements might necessitate a departure from that section, as in the case of a tunnel recently constructed by him in Dublin, or in soft silt and similar strata it might be advisable to adopt an oval section, and in such cases there would not be the difficulty Mr. Moir apprehended in carrying out that section with a shield. The fact that the shield in its progress had a tendency to rotate upon its axis was not a reason for adopting the circular form, but, on the contrary, it would rather be a reason, though not in itself a sufficient one, for a departure from that form, because rotation of the shield would then be prevented by the iron lining. There was no mystery about the cause of the rotation. It was undoubtedly due to the action of the hydraulic presses or screws (it had occurred in the Tower Subway shield), and resulted from their combined thrust being out of parallelism with the axis of the shield. He had suggested as a remedy that the presses or some of them should be so attached to the shield that their direction could be adjusted slightly to counteract any tendency of the shield to rotate. The taking out of iron and substituting concrete for it in the lining of the tunnel had often been considered; but he was satisfied that, with the present price of iron at any rate, that would certainly not be an economical method of proceeding in London, and it would lead to considerable loss of time and some risk, in most cases, of injury to property. There were localities, no doubt, where concrete would be more economical than iron, but it would probably be found best in such cases to build the tunnel of concrete in the shape of moulded segments or blocks, thoroughly set and hardened before use. Concrete in this form, unlike "green" concrete, would be capable at once of resisting pressure and of excluding water. The unfortunate catastrophe in Melbourne had been referred to by Mr. Moir. He (Mr. Greathead) had not been consulted about that tunnel, but he heard that the contractors for that work had sent to some merchants in London for a shield, and

Mr. Greathead. that a shield had been obtained made from the designs of the City and South London shields. It had been sent out and set to work. He hardly thought he should be held responsible for what might happen under those conditions. A shield was not a thing to be ordered as a plough, for every tunnel had to be treated in a special manner. With regard to the hydraulic erector referred to in the Paper, all the drawings, not only of the erector, but of the shield and other parts of the tunnel at Woolwich, designed between 1874 and 1876, could be seen.

There was no suggestion in the Paper that brick air-locks should be used under all conditions and sizes of tunnels; and the question was not so much one of cost as of the health of the men, who, both on the City and South London Railway and on the Waterloo and City Railway, had a decided preference for the brick over the iron air-locks. The possibility of taking a lock forward through another lock was, in driving long tunnels, also a considerable convenience. Similarly, it was not suggested, as Mr. Moir assumed, that in a large tunnel the shield should be regarded as a substitute for safety screens in the tunnel. In small tunnels it was not possible to introduce hanging screens; but, in any tunnel large enough to admit of it, at least one screen should be provided and carefully maintained air-tight, and, wherever practicable, the road in rear of the screen should at one point be elevated to a level above that of the lower edge of the screen by inclines in both directions, and carried over a watertight bank in the invert. To avoid the inconvenience of the gradients, it was quite feasible to take the road through a gate in the bank, closing fairly watertight, and kept closed by springs or otherwise, except at the time of the passage of trucks, &c. Even when a tunnel was being driven on an ascending gradient, and where a simple hanging screen might be useless, this arrangement would secure the whole of the air-locks in the bulkhead against the possibility of being submerged. It was suggested by Mr. Moir that the tunnels in the London clay should be driven in the same way as the Hudson Tunnel had been, and that, in tunnelling through gravel under streets, the same mode of attack should be adopted as in the Blackwall Tunnel. Under all conditions the shield should, no doubt, be made to do as much of the mining as possible; but in tunnelling, as in other matters, different cases required different treatment, and London clay could not be dealt with precisely in the same way as soft silt, nor was it feasible to let down the surface of the streets in the way that the bed of a river might be. It was of the first importance, in driving through

the loose water-bearing strata on the City and South London Railway, to avoid the slightest disturbance overhead, and that condition had been fully attained by the method employed. Mr. Greathead.

No special castings were used in the City and South London tunnels in driving round curves, the difference in length between the inner and outer circumferences being made up by filling in the joints; but on the sharper curves of the Waterloo and City Railway special castings had been introduced. The adjustable cutters of the shield, described in the Paper, had been found to act admirably, when properly regulated, in tunnelling round curves. As the tunnel under the Seine at Clichy had been referred to, he might perhaps be allowed to state that the shield for that work had been constructed in this country from his (Mr. Greathead's) designs, and that his compressed-air grouting apparatus had also been employed there. The cost of this tunnel, as given by Mr. Fitzmaurice, viz., £29 per foot, appeared to be very great for a diameter of only 7 feet 7 inches. The contract price quoted (p. 83) from Mr. Parsons' report as that of the Waterloo and City Railway tunnels, viz., £32 per foot of double tunnel, was for the tunnels constructed under compressed air through water-bearing gravel. The price for the tunnels in clay was about £24 per foot of double tunnel.

The use of shields in clay had been referred to by Mr. Binnie, who seemed to think that they were not of much use there except as supports to avoid the use of timber. He (Mr. Greathead) took a very different view. He was quite satisfied that the London tunnels could not have been constructed without shields, even in clay. There was the important question of speed. Unless the tunnels could be constructed at great speed, it would be necessary to have temporary shafts in the streets; and it was known what the London County Council would say to a proposition to place temporary shafts along Oxford Street, for example. He did not think that a tunnel could be constructed with perfect safety without a shield. When passing through clay, a shield was an assurance against other things that might be met with. The shield also led to a considerable reduction of labour. The introduction of the wedges in front of the shield, which threw upon the hydraulic presses some of the work of excavation, had led to a very material acceleration of the work and a corresponding reduction in cost. In fact, in the City and South London tunnels the introduction of those wedges had almost doubled the speed in constructing the first tunnels. That was a very important matter, in view of the present price of labour. In stating that the City

Mr. Greathead. and South London and the Waterloo and City tunnels were as easily constructed as would be a tunnel under Primrose Hill, Mr. Binnie had overlooked the fact that in both those cases compressed air had to be employed within a few yards of the Thames to keep back the water from the river. He need not follow Mr. Binnie through his remarks on the Brunel patent, which was described in the Paper, except to say that, in common with all engineers, he had the greatest admiration for Brunel, and that if he had said in the Paper that the Thames Tunnel had acted as a warning to engineers, he had done no more than state a fact. As to the Cochrane patent (also described in the Paper), it was really a patent for air-lock arrangements, and for certain arrangements for removing excavated material; it was not a patent for tunnelling through loose water-bearing strata under compressed air. The drawings produced by Mr. Binnie showed a section of a tunnel in clay, and the end of the tunnel was perfectly open. No doubt Lord Dundonald contemplated the use of compressed air in tunnelling, but he had not devised any means by which it could be used in loose or open water-bearing strata.

Surprise had been expressed by Mr. Crawford Barlow that reference had not been made at all to the work of the late Mr. Peter Barlow. It was impossible within the limits of a single Paper to describe fully all that had been done, much less, every patent that had been taken out, but as a former pupil of Mr. Peter Barlow, he had desired to give due prominence to his work, and the Paper contained a description of the Tower Subway and of Mr. Barlow's Omnibus Subway scheme. He had not thought it necessary to refer to the Southwark Subway scheme, in which he had been associated with Mr. Barlow. It was an extension of the Tower Subway idea and embraced a small tube 8 feet in diameter and 1,200 yards long, with a lift for twenty passengers at each end, and carriages propelled by a wire rope and two stationary engines. After the failure of this arrangement at the Tower Subway, it was found impossible to proceed with the undertaking. Until Mr. Crawford Barlow stated that a patent had been taken out by his uncle in 1868, he (Mr. Greathead) had not known of it, and that was no doubt due to the fact that Mr. Barlow attached so little importance to the subject that he did not proceed beyond the preliminary stage with his application for a patent. The shield used at the Tower Subway was described in the Paper and was not as represented in either of the figures presented by Mr. Crawford Barlow.

As to the question of Mr. Lewis whether the grouting had been

tested, the fact that at the stations about 2,000 feet in length of the tunnels had been taken out was referred to in the Paper, and the perfect condition of the grouting behind the iron lining described. Within half-an-hour of its injection the grouting was generally as hard as the clay, and in a short time it became much harder, and it was therefore capable of taking the pressure transmitted by the clay. The cases of damage to property on the railway had been occasioned by the construction of the brick-lined tunnels, and were not due to any enlarged iron tunnels. As regards pumping, the success of the system of tunnelling described was due to the avoidance of that source of expense and danger. Mr. Greathead.

Correspondence.

Mr. JAMES ARMER stated, with reference to the tunnelling in connection with the metropolitan sewerage system, under the River Yarra in Melbourne, that, a former section of the work having been abandoned on account of the difficulties in driving through the silt formation, a shield had been made and sent from England early in 1894. It was 11 feet in diameter, and was in other respects identical to those employed on the City and South London Railway. The length adopted for the rings of the tunnel lining had been 2 feet 9 inches. During August, 1894, the rate of advance by this shield had been 11 feet 8 inches per shift of eight hours, an increase of 18 inches in the rate being anticipated with the addition of segment-lifting arrangements. The progress made in the same time by a shield constructed in the colony and working under similar conditions had been 2 feet 9 inches. The work under the Yarra was of a more dangerous character than that described in the Paper; but the accident in April, 1895, would appear to have been in no way attributable to the shield. Mr. Armer.

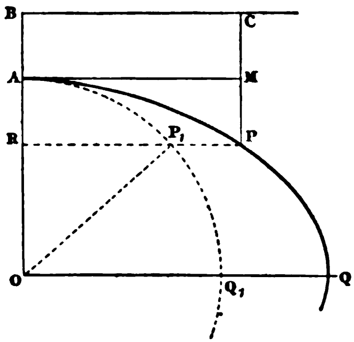
Mr. C. J. BELLAMY observed that the "hydrostatic arch" was well known to be the form suitable for sustaining the pressure of a perfect fluid, provided that its weight might be neglected, as being very small compared with the pressure of the fluid. This arch was not a closed curve, but a succession of loops. If, however, the weight of the arch itself was taken into account the curve became considerably modified, and there was a particular relation between the weight of the arch and the pressure of the fluid, for which the curve became a circle, as would be shown. In *Fig. 32* A P Q was a cross-section of the semi-arch at right angles to its length, and B C the surface of the fluid. The vertical forces Mr. Bellamy.

Mr. Bellamy. which kept the portion of the arch A P in equilibrium were the weight of the fluid A B C P, the weight of the portion of arch A P, and the vertical component of the thrust at P. Hence, equating the resultant of these forces to zero—

$$\int_{0}^{x} w(a+y)\,dx + vs - t\frac{dy}{ds} = 0 \quad . \quad . \quad . \quad (1)$$

where A B = a, A M = x, M P = y, A P = s, w = weight per cubic unit of fluid, v = weight per square unit of arch surface, t = linear thrust at any point P per unit length of arch, and T = linear thrust at A the highest point of the arch, where the curve was horizontal. The horizontal forces acting on A P were the thrust at A, the

Fig. 32.



horizontal pressure of the fluid on the vertical plane M P, and the horizontal component of the thrust at P. Hence

$$T - \int_{0}^{y} w(a+y)\,dy - t\frac{dx}{ds} = 0 \quad . \quad . \quad . \quad (2)$$

Differentiating equations (1) and (2) with respect to s—

$$w(a+y)\frac{dx}{ds} + v - \frac{dt}{ds}\cdot\frac{dy}{ds} - t\frac{d^2y}{ds^2} = 0 \quad . \quad . \quad (3)$$

$$-w(a+y)\frac{dy}{ds} - \frac{dt}{ds}\cdot\frac{dx}{ds} - t\frac{d^2x}{ds^2} = 0 \quad . \quad . \quad (4)$$

Multiplying (3) by $\frac{dy}{ds}$ and (4) by $\frac{dx}{ds}$ and adding them together the resulting equation became—

$$v\frac{dy}{ds} - \frac{dt}{ds} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Mr Bellamy.

therefore $\frac{dt}{dy} = v$,

and $$t = T + vy \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Inserting this value of t in equation (2) and performing the integration in that equation, there resulted—

$$\frac{dx}{ds} = \frac{T - \frac{w}{2}(a+y)^2 + \frac{w}{2}a^2}{T + vy} \quad . \quad . \quad . \quad (7)$$

If b be the value of y, when the curve became vertical as at Q, at which point $\frac{dx}{ds} = 0$,

$$T = \frac{w}{2}\{(a+b)^2 - a^2\} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Substituting this value of T in equation (7)—

$$\frac{dx}{ds} = \frac{(a+b)^2 - (a+y)^2}{(a+b)^2 - a^2 + \frac{2v}{w}y} \quad . \quad . \quad . \quad . \quad (9)$$

If now a particular value be given to v, viz., $\frac{wb}{2}$, equation (9) reduced to—

$$\frac{dx}{ds} = \frac{b-y}{b} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

If in *Fig. 32* the horizontal line QO be drawn, and with O as centre the circle AP_1Q_1 be described with radius OA, and the horizontal line PP_1R drawn, then at the point P_1 on the circle

$$\frac{dx}{ds} = \cos AOP_1 = \frac{OR}{OP_1} = \frac{b-y}{b};$$

therefore the curve of the arch APQ coincided with the circle AP_1Q_1. This particular value which had been given to v, viz., $\frac{wb}{2}$, implied that the weight of any portion of the circular arch as AP_1 was equal to the weight of fluid corresponding to the sectorial area AOP_1, so that if the curve comprised the entire circle the weight of the cylinder was equal to that of the fluid it displaced; and with this condition the circular cross-section was a curve of equilibrium; and there was no bending stress on the cylinder, whatever be the depth of its immersion.

Mr. Brightmore. Mr. A. W. Brightmore wished to add a few details in connection with the Vyrnwy Aqueduct tunnel under the River Mersey. The tunnel shafts were built of cast-iron, and were 800 feet apart from centre to centre. The depths of the shafts to the invert-level on the Cheshire and Lancashire shores of the river were 46 feet and 52 feet respectively. The external diameter of the tunnel was 10 feet, and that of the shafts 10 feet 9 inches, the latter being widened at the bottom to 15 feet. The minimum height of the river-bed above the tunnel was 20 feet, and the invert-level was about 50 feet below high water. The shafts were built of flanged cast-iron rings, 4 feet deep, varying in thickness between 1 inch and $1\frac{1}{16}$ inch, down to the widened part, which was built of smaller segments. The tunnel was built in rings, in each of which were ten segments, 3 feet by 1 foot 6 inches and $1\frac{1}{16}$ inch thick, weighing 4 cwt. each, and a key-segment, 1 foot by 1 foot 6 inches, weighing 1 cwt. The shafts had been sunk by loading the cylinders and excavating with a grab in the ordinary way, and nearly four-fifths of the tunnel had been driven by means of an improved shield; ten hydraulic rams, 7 inches in diameter, and working at a pressure of 1,000 lbs. to 2,000 lbs. per square inch, being used to force it forward. As the shield had usually a tendency to droop at the cutting-edge, it had been seldom necessary to employ the upper rams—a circumstance, no doubt, due to the greater resistance at the bottom of the shield. When there was no unusual obstruction, the friction in the shield amounted to about 4 cwt. per square foot, and sometimes to somewhat less.

The pressure of air in the tunnel was usually between 12 lbs. and 15 lbs. per square inch above the atmosphere, often about two-thirds the hydrostatic pressure of the water outside. As a rule it had been necessary to pump into the tunnel 500 cubic feet of air per minute (measured at atmospheric pressure), and in exceptional cases more than double that amount. Two methods of working had been employed. When, owing to the fairly uniform permeability of the strata, the air escaped evenly through the face and kept it dry, except quite at the bottom, the men, while standing in front of the diaphragm, excavated for about 18 inches beyond the cutting-edge; the pressure being then turned on to the rams, the shield advanced into the space so prepared. When the air escaped unevenly, the face, or a portion of it, was wet, and the men excavated standing in the pocket between the two diaphragms, the pressure being kept on the rams, so that the shield crept forward as each spadeful of material was removed, until it had advanced 18 inches. The time taken to advance the

shield this distance and get out the excavation was practically the same with both methods. The next ring of segments was then fixed for the excavation to be resumed as before. It was worthy of remark that, after the improved shield had been put to work, the time taken for the excavation and advance of the shield was fairly constant, at the rate of about one hour and a half for each ring, unless there was some unusual obstruction. On the other hand, the fixing of a ring of segments took at first four hours; but when the men were well drilled to the work, this rate was greatly increased, and before the completion of the tunnel it had been found possible to fix a ring in one hour and a half. The leakage of water into the tunnel during the driving had varied between 30,000 gallons and 40,000 gallons per day. Mr. Brightmore.

Mr. E. G. Carey remarked that the rapid development of the system of tunnelling described in this Paper had led to the design of special plant, for the manufacture of the cast-iron segments of the lining. Those for the Glasgow District Subway were almost identical with those for the City and South London Railway, and were moulded in specially designed machines consisting of a 4-inch hydraulic ram working both the pattern and the box. A pair of side levers, actuated by a second hydraulic ram 2 inches in diameter and of 11 inches stroke, held the swing-head in position, the latter consisting of a cast-iron girder, with a hard wood ramming-block bolted to its lower side. Four guides, 4½ inches in diameter, were secured to the cast-iron bed-plate to direct the actions of the ram which was counterbalanced. The empty moulds were conveyed beneath the machine on bogies and were transferred to it by the outline plate which was swung over. The ram then brought the pattern into place, sand being fed in from a hopper above, and the swing-head was then raised, and the ram caused to bring a final pressure to bear on the sand. The ram was then lowered, withdrawing the pattern and the swing-head being thrown back, the mould was turned over on to the bogie beneath by the outline plate. A hand-screw withdrew the pattern ends from the mould. The hydraulic pressure was 1,500 lbs. per square inch. Each of these machines produced 1½ ring per hour (9 segments per ring). The total quantity in the Glasgow District Subway was 20,000 tons. Mr. Carey.

The Blackwall Tunnel segments had been machine-moulded, their size prohibiting hydraulic moulding. The moulding-machine consisted essentially of a main framing of cast-iron, carrying both pattern and moulding-box, and capable of turning completely over on side hinges. The pattern flanges were operated by four hand-wheels, 24 inches in diameter, to enable the mould to leave the

Mr. Carey. pattern, which was of mahogany, brass bound at the edges. The pattern with flanges in position, and mould-box being in place, sand was shovelled in and rammed, the lid was then clamped on and the main framing thrown completely over. The flanges were then withdrawn and the mould received the hand-moulded top portion. This machine would make between thirty and thirty-three segments in the working day of nine and a half hours, the maximum attained in this time being thirty-six segments. The segments of the Edinburgh Mound Tunnels, and the Glasgow Central Railway sewers, were hand-moulded, the quantities required not warranting the construction of special plant. Special milling plant had been designed for machining the ends and sides of the segments. Two types had been adopted, viz., the work stationary and the tool travelling, and *vice versâ*. The milling heads, whether travelling or not, were essentially the same in each type and were furnished with cutting tools about 3 inches apart on the circumference. More than 50 miles of milling of 10 inches wide planing was thus performed. The patent moulding and milling plant had been constructed from the designs of Mr. Stephen Alley, Glasgow, at the works of the British Hydraulic Foundry Company, Limited, Whiteinch, Glasgow.

Mr. Fox. Mr. Francis Fox, of Westminster, had had considerable experience with the Greathead shield and grouting machine, and desired to bear his testimony to the great value of these appliances, which together formed one of the greatest improvements that had been introduced into the art of tunnelling during the last thirty or forty years. They had, to a great extent, deprived subaqueous or submarine tunnels of the risk formerly attending their execution, and had also, under many circumstances, much reduced their cost. Such tunnels could now be driven safely and rapidly through silt, gravel and sand, previously impassable at any reasonable cost. It might be true that the idea of a shield was not a new one; and the late Sir Charles Fox, M. Inst. C.E., had employed cement grout under hydrostatic pressure for the repair of cracks in brickwork, as early as 1865. The system of blowing in the grout by compressed air, introduced by the Author, was, however, a great improvement upon the hydrostatic process; and, having employed it upon several occasions, and under varying circumstances, he had formed the highest opinion of its efficiency, and commended it to the attention, not only of engineers, but of architects, for the grouting of masonry or brickwork showing cracks due to settlement, or other causes. For the repair of cathedral or church towers, or tall chimneys, he considered that it would prove

of the greatest value, the cost of its efficient application to ensure the safety of the structure being probably not one-fiftieth part of the expense which would be incurred in rebuilding. He would not attempt to express an opinion as to the probable originator of a shield, for which several claimants had been put forward; but he considered that the credit of introducing, practically applying, and working out to a satisfactory completion the system under review was undoubtedly due to the Author, and that his name would be inseparably connected with this most important addition to the resources of civil engineers engaged in the construction of tunnels. Mr. Fox.

Mr. DRUITT HALPIN observed that a useful Table of costs of construction of various underground railways was given in the Zeitschrift des Vereines deutscher Ingenieure of the 26th October, 1895. Mr. Halpin.

Mr. J. C. HAWKSHAW considered that the process of tunnelling by means of a shield and compressed air, which had been so successfully used and so well described by the Author, was, and always would be, of great value in driving tunnels through water-bearing strata at no great depth below the surface of the water which determined its pressure. But the limiting depth at which the shield process could be used was soon reached; probably 80 feet would be a maximum depth. The cylinders of Londonderry Bridge had been sunk by compressed air under a water-pressure of 80 feet, but five men had lost their lives owing to the severity of the work under such a pressure. That the process described in the Paper could be used only in tunnels at a comparatively shallow depth must be sufficiently obvious. It had been inferred that the Severn Tunnel could have been, or could now be, more cheaply constructed by using a shield with compressed air, and that similarly the Channel Tunnel and the Irish Tunnel could be more cheaply constructed by the same means. Compressed air could not be used in connection with a shield to keep back water at the tunnel faces in any one of the three cases mentioned. The least depth of the rail-level below high water, in the Severn Tunnel, was 80 feet close in shore; and it increased to 160 feet below the shoots and to 200 feet at the drainage heading. In the English Channel there was a depth of 120 feet of water, and the tunnel itself would have to be at a much lower level, probably between 250 feet and 300 feet below the surface of the water. In any tunnel to Ireland the depth would be greater still, as there was a sounding of 500 feet in the channel between the two countries. In the cases of the three tunnels mentioned, therefore, the process Mr. Hawkshaw.

Mr. Hawkshaw. adopted with such success on the City and South London Railway could be of no avail. If a second Severn Tunnel were made, all the water penetrating into the working would have to be pumped, and so it would be if tunnels were made beneath the channel between England and France, or between England and Ireland. The practicability of making such tunnels depended on whether the whole of the water entering the workings could be dealt with by pumping. If it could not there was no known process by which such tunnels could be made.

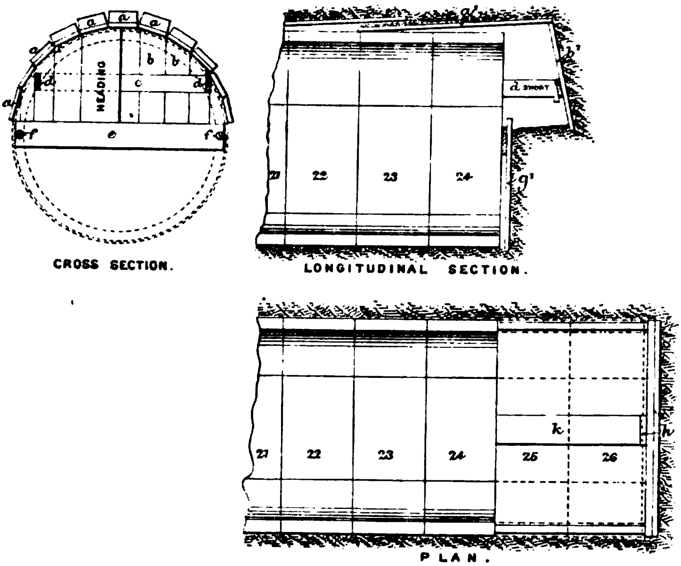
Mr. Hay. Mr. David Hay stated, with reference to the alignment of the tunnels described in the Paper, that, in order to determine their exact positions, an accurate survey had been made, showing the main roads to be followed and the buildings fronting upon them. This survey having been laid down to a scale of 10 feet to an inch, the centre lines of the tunnels had been marked, and points in the straight portions measured off from prominent buildings. These points had been established in the road, either by driving thin steel wedges between the setts where the road was paved, or by iron pins 12 inches long where it was macadamised, a centre-punch mark being made in either case. The lines had then been measured to the nearest curb, so that the marks might be easily found or reinstated if destroyed. The base lines thus fixed had been extended, and where they intersected each other the angle had been read off with a theodolite, the lengths between the points of intersection being carefully measured. In consequence of the heavy vehicular traffic all along the route of the railway during the day, it had been found possible to do this work only at night or early on Sunday mornings. In the former case a tissue-paper screen, held in front of an electric lamp driven from a primary battery, had been found most useful for sighting. In consequence of the shafts having been at some distance from the tunnels, the lines fixed on the surface could not be directly transferred to the tunnel. It had therefore been necessary to ascertain the angle between the line of the cross-passage and the line of tunnel. After the shaft had been sunk, the line of the cross-passage had been set out on the surface and the point of intersection, with its angle, fixed and read in the usual way. Two plumb wires had then been sighted into line in the shaft, to which the cross-passage had been driven the requisite length. The wires had been again sighted, and the distance, from the intersection to one of them, accurately measured on the surface. The theodolite had then been taken below and set up in the line of wires at a distance from them corresponding with that measured on the

surface, and the angle read off, thus determining the line of tunnel to be driven. This operation had been repeated two or three times at each station until a sufficient length of tunnel had been driven to admit of a reliable base line being established. The most convenient method of establishing points had been found by filing nicks in staples driven into hard-wood wedges fixed between the cast-iron rings at the top of the tunnel. When sighting a plummet line in the staple, the tissue-paper screen had been placed behind it; and when the atmosphere in the tunnel was clear extreme accuracy had been possible, the smallest variation of the line being easily detected. A clear sight, however, could seldom be obtained with the theodolite for more than 250 feet or 300 feet, on account of the smoke from the candles, &c.; recourse had therefore to be had to a long steel piano-wire $\frac{1}{32}$-inch thick to produce the straight lines. This wire had been sometimes used for lengths up to 700 feet and had been stretched along the roof of the tunnel out of the way of traffic. By the use of plummets with fine steel points and thumbscrews for adjusting the wire great accuracy had been obtained, and after the first base line had been established it had never been found necessary to use the theodolite until a curve was reached. The line staples had been driven about every fifty rings, but never close to the shield, as there was a danger of the lining moving slightly. Daily checkings of the portion of the lining fixed had been made with a shorter wire, one end being attached to the third staple from the face, and the other end fastened to the shield. It had then been adjusted by the other two staples, a light plummet line being hung upon it opposite the last ring, and measurements being taken to check the position. A 7-inch theodolite had been used by Troughton and Simms. No special rings of cast-iron lining had been used for the curves; consequently iron packings had to be placed between the rings to make the joints radiate from the centre point of the curve. When approaching a curve the side of shield on the outside was made to lead a distance corresponding to the iron packing to be inserted, and at the same time the cutting edge of shield was freed on the inside of curve. This latter was a very important point and must be insisted upon, otherwise the men would neglect it, as it gave them a good deal more trouble. Mr. Hay.

Mr. W. A. P. Tait observed that the tendency appeared, from Table II of the Appendix, to be in the direction of an increase in the internal diameter of subways for electric or cable railways. It would be useful if the Author would not only state the cost of the lines of 10 feet 2 inches and 12 feet 6 inches internal diameter, Mr. Tait.

Mr. Tait. but the probable cost in similar circumstances of lines large enough to take the ordinary railway rolling-stock and satisfy Board of Trade requirements. It appeared a short-sighted policy, even though there might be a small saving, to construct subways of such small diameter as to cause the equivalent of a break of gauge. The permanent way was of the ordinary gauge, but there could not be reciprocal running powers. He understood that the air of the District Railway contained between twenty-two and thirty parts in 10,000 of carbonic acid, a somewhat larger ratio than found in an

Figs. 33.



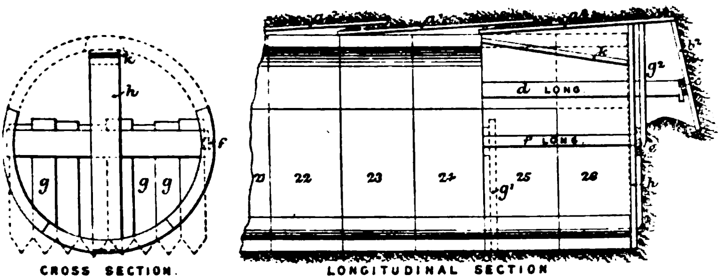
Scale, ¼ inch to 1 foot.

average theatre. Analyses of the air in the tunnels worked by the electric engine would be interesting.

Regarding the driving of the tunnels, he had had charge during construction of two of the small tunnels under the streets of Glasgow referred to by the Author. One of 4 feet internal diameter had been driven through wet sand, gravel and ballast, without a shield, and had proceeded at the rate of 1½ yard daily at each face. The progress of the other with a shield had been less satisfactory. It had been usual to take out, at one time, excavation for two complete rings of cast-iron, each made up of six segments, representing an advance of 3 feet. Eleven poling boards, *aa*, *Figs. 33*, supported at their

leading ends by *bb*, had been used to support the roof until the two rings of the lining were finally erected. The other ends of the polings had been supported on the last completed length. The crown polings rose 3 inches in their length forward to allow for subsidence, and still permit the placing of the poling for the next length beneath. In order to "get in the top" for the rings Nos. 25 and 26, for example, the miner first removed a leg *b*, under a crown poling a^1 and drove a heading, shown in the section, the full length, about 4 feet, of the new poling board, a^2, and immediately propped it with the old leg, b_2, *Figs. 33* and *34*, and so on, widening the heading out to either side until all the new polings, a_2, were inserted and supported by legs, b_2. Great care had been taken to cover each of the polings before they were inserted with a layer of well-tempered clay. This had occupied the time of two miners, who in the limited space could only work Mr. Tait.

Figs. 34.



Scale, ¼ inch to 1 foot.

alternately, and a labourer about four hours. The legs, b_2, were then secured by a waling *c*, *Figs. 34*, held by two rances, *d d*, butting against the flange of the ring last erected. Next a waling *e* (held in same way) was set up so as to be somewhat more than 3 feet from the last ring, in order to let in two rings of 18 inches each. A series of piles, g_2 and *g*, *Figs. 34*, were set up and worked home as a little excavation was taken out. Little by little the dumpling had been removed, until at last one half of the length had been bottomed. Some difficulty had occasionally arisen at this stage, if the air-pressure was not maintained, and great care was necessary in taking out the last of the muck. In such a case the bottom plates had been laid in water. This second stage had occupied about three hours. After the bottom segment had been inserted, the trouble was much reduced, and the work became more easy. Before the second segment from the bottom could be

Mr. Tait. inserted, one of the rances, *f*, had to be removed, but it had been temporarily replaced by the soldier and stretcher, *h* and *k*, *Figs. 33* and *34*, but as soon as the second segment of ring 26 had been inserted, the waling, *e*, had been dwanged from the flange of that segment. The subsequent grouting had been as described by the Author. While all the polings used were allowed to remain, the rest of the timber was used again and again, the long and short pairs of rances *d* and *f* being used alternately.

A curious paradox in connection with the driving of tunnels in compressed air under the River Clyde had been noticed, viz., that a lower pressure often sufficed to keep the tunnel dry at high than at low water. This had been possibly due to consolidation of the silt by the increased superincumbent pressure. In view of the fact that excavation at a working face, unless supported, did not stand vertical, it occurred to him some years ago that in order to effect greater progress with a shield, the roof should be prolonged slightly so as to take the natural slope. This would, to some extent, obviate the present practice of working in advance of the shield; but care would have to be taken to prevent the shield from turning about its axis, a circumstance which occasionally happened.

Mr. Greathead. Mr. Greathead, in reply to the correspondence, said that in fixing 80 feet as the maximum depth under water for tunnelling by shield and compressed air, Mr. Hawkshaw was not far wrong if he meant that the pressure of the air was to be, as usually in shaft sinking, such as to completely balance the head of water; but there were occasions when this was by no means necessary, and where compressed air could be used in tunnelling at much greater depths than 80 feet with great effect as regarded safety and economy. There could be no doubt that in cases where much water was encountered at great depths, whether compressed air were used or not, it would often be economical to put in cast-iron lining to avoid the permanent cost of pumping. The reduced quantity of excavation would generally in such cases balance the extra cost of iron over brickwork. It was stated by Mr. Carey that the cast-iron segments for the Glasgow District Subway were almost identical with those of the City and South London Railway. The design of the latter had, however, been departed from in the former in one respect which it was worth while to notice. The longitudinal joints, instead of being made with flat faces of the whole depth of the flanges, Fig. 16, Plate 2, had been made like the vertical joints in the City and South London tunnels, with a chipping edge or fillet along the outer edge of the flange. Where

rigidity was required to prevent deformation and to secure the maximnm efficiency of the iron this form of longitudinal joint, with a soft-wood packing between the flanges, appeared to him to be an unfortunate modification, because the bearing-surface of segment against segment was practically reduced to the width of the chipping edge with the result that there was very little resistance to outward movement, away from the centre, at any of these joints. Mr. Greathead.

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26 November, 1895.

Sir BENJAMIN BAKER, K.C.M.G., President, in the Chair.

The discussion of the Paper on "The City and South London Railway" occupied the evening.

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3 December, 1895.

Sir BENJAMIN BAKER, K.C.M.G., President, in the Chair.

It was announced that the several Associate Members hereunder mentioned had been transferred to the class of

Member.

Onward Bates.
William Thomas Clifford Beckett.
Arthur Sackville Boucher.
Robert Edden Commans.
Charles Friend Cooper.
George Estall.
John Coleman Fergusson.
Alexander Maurice Lane.
Donald Alexander Matheson.
Walter Parry.
Kenneth Reid.
Charles de Grave Sells.
Charles Walter Smith.
George Frederick Tippett.

And that the following Candidates had been admitted as

Students.

Bertram Keith Adams.
Harry Walton Appleby.
Frank Baker, B.Sc.
Reginald Francis Baker.
William Ralph Baldwin-Wiseman, B.Sc.
Edward Alexander Morgan Bindloss.
Lawrence Birks, B.Sc.
Alfred Brown Ernest Blackburn.
Archibald Thomas Bostock.
Joseph Henry Draper Brearley, B.Sc., B.E.
James Hutchieson Brown.
William Lowe Brown, B.Sc.
Arthur Burton Buckley.
Hamilton Amyatt Bullock.
Thomas Carter.
Alfred Eustace Chambers.
Charles Randolph Chater.
Moreton John Godden Colyer.
Vernon Cooper.
Herbert Sydney Culverhouse.
Walter Daniel.
Benjamin John Day.
Claud Albert Stainton Day.
Robert Ferry Drury.
Francis William Duquemin.
Frank Rogers Durham.
Frank Fisher, B.Sc.
Charles Douglas Gee.
Alfred Grice Godfrey.
William Gore.
Alistair Mann Grant.
William Brasier Hall, R.N.
Alfred Woods Hanckel, B.Sc.
Henry Arthur Hardcastle.
John Russell Hart, B.A.
Alexander Herbrand Harvey.
Stanley St. George Harvey.
Joseph William Hayward, B.Sc.
Bertram Henry Majendie Hewett
William Hugh Hogarth.
Algernon Hoyle.
Francis William Richard Hurt.
William Mangles I'Anson.
Daniel Jaffé.
Christopher Jakeman.
Cyril Mure Jeffcoat.
John Alexander Jones.

Students—continued.

George Alfred Julius.
Robert Graham Keevill.
Rennie Malcolm Kerr.
Henry McKenzie Kirkby.
John Charles Staveley Lawson.
George William Leeson.
Arthur Wilfrid Lewis.
Frederic Horace Faviell Lucas.
William Henry Mahon.
Thomas Tyson Middleton.
Harold Wood Milner, B.Sc.
William Henry Moorby, B.Sc.
Magnus Mowat, Jun.
Thomas Edmund Pallant.
Henry Charles Penny.
George Stephens Perry.
Robert Henry Pocklington.
Handel Norman Pope.
Frederick Kennerley Preston.
William Alexander Rain.
Charles Edwin Rivers.
Henry Robinson.
Ralph Robinson.
David Ronald.
Lewis Henry Rugg.
John Aloysius Ryan.
Reginald Arthur Ryves.
Charles Emile Herbert Salmon.
Charles Phillips Sanders.
William Henry Bourchier Savile.
Louis Sikes, B.Sc.
Hugh Wyatt Standen.
Bernard Stephenson.
Grote Stirling.
Englebert Hugh van der Straaten.
Basil Stigand Streeten.
Frank Donald Stuart.
Harold Lionel John Swindlehurst.
Ernest Arthur Tarrant.
Charles Kenneth Trubshaw.
Reginald Stanley Underhill, B.A.
Thomas Arthur Wethered.
Alfred Carruthers Wilson, B.Sc.
John Sigmund Wilson.
Guy Peters Wincey.
Peter Bisset Woodger.

The Candidates balloted for and duly elected were: as

Members.

George Robert Bardsley.
William George Bligh.
Henry Hargrave Deane, B.A.
James Hennen Jennings.
Charles Perrin.

Associate Members.

Charles George Heber Ackland.
William Alexander, Stud. Inst. C.E.
Henry Newmarch Allott.
James Edgar Askwith, Stud. Inst. C.E.
Thomas Barker.
Hugh Bateson.
Ernest Albert Seymour Bell, F.C.H.
James Bell.
Leonard Moore Bell.
Walter Blackshaw.
John Barker Bradshaw.
William Joseph Bremner-Davis, Stud. Inst. C.E.
John Boyd Brodie, Stud. Inst. C.E.
Joseph William Buckley.
Edward Lancaster Burne, Stud. Inst. C.E.
Edward Saville Burrough, Stud. Inst. C.E.
Francis Furlong Byrne.
Edwin Kitson Clark, M.A.
Albert Henry Claypoole, Stud. Inst. C.E.
Richard Haworth Clayton.
Walter Hepworth Collins.
Alfred Chorley Cookson.
Richard Edward Synge Cooper.
William Robertson Copland, Jun.
Walter Robinson Crabtree, M.Sc., Stud. Inst. C.E.
Bernard Humphrey Crookes, M.Sc.
Brysson Cunningham, B.E.
David Crawford Dick.
Ernest Harry Druce, Stud. Inst. C.E.
Robet Gordon Edgell.

Associate Members—continued.

William Clark Fisher.
Alfred Sigismund Frech.
Edward Garside.
William Henry Hamer.
Henry Lipson Hancock.
John Parker Harris.
Frederick Henry Hatch, Ph.D.
Herbert Heap.
Gilbert Reginald Henderson, Stud. Inst. C.E.
Harry James Hills, Stud. Inst. C.E.
Reginald James Hinton.
Adam Hunter.
William Gallon Hunter.
William Haines Hutchinson.
William Hutton.
John Hyslop.
Frederick Alexander Jamieson, Stud. Inst. C.E.
Henry Japp.
Thomas Kennedy, Jun.
John Frederic Charles Kimber, Stud. Inst. C.E.
James Thomas Landless.
Charles Edward Larard.
David John Lavelle.
Cyril D'Arcy Leaver, Stud. Inst. C.E.
Herbert Lees.
Albert George Little.
Arthur Brincken Matthews, B.A.
Domingo Merry del Val y Zulueta, Stud. Inst. C.E.
Harry James Molloy.
Edwin John Moon, Stud. Inst. C.E.
George Nelson.
Alexander Newlands.
Richard Soulby Oldham.
Joseph Charles Pardoe.
Alexander Francis Pearse.
Gerrard Hatfield Perryn.
George Henry Pickles.
Herbert Pilkington.
Robert Albert Powell.
Edward John Prew.
Walter Cromwell Price, B.A.
John Provis.
William James Quodling, Stud. Inst. C.E.
Alexander Reid.
Keith Robinson, Stud. Inst. C.E.
William Ross, B.E.
Harry Lionel Sargent, Stud. Inst. C.E.
William Savage, Wh.Sc.
Leopold Halliday Savile, Stud. Inst. C.E.
Thomas Earle Scaife.
Arthur James Scratchley.
Alexander Shand.
George William Smith.
William Warren Smith.
Philip Bertie Spencer-Stanhope, B.A.
Gilbert Stoker.
Sidney Straker.
James Sutherland.
John Bligh Suttor.
James Talbot.
John Thompson.
Richard Threlfall.
Herbert Williams Umney, Stud. Inst C.E.
William Carlile Wallace.
Stanley Dawson Ware.
George James Wells, Wh.Sc.
Francis Edmund Wickham.
George Charles Bradley Wieland.
Arthur Edward Williams, B.A., Stud. Inst. C.E.
Charles Beynon Williams.
Herbert Morton Willmott, F.C.H.
Frederick James Wilson.
Norman Forster Wilson.
James Herbert Wood.
Isidore Michael Xavier, Stud. Inst. C.E.
Victor Hansard Yockney.

Associates.

Frederick Mortimer Barwick, *Capt. H.M. Indian Marine.*
George Burt.
William Robert Freeman.
James Aubrey Gibbon, *Capt. R.E.*
John Barber Lindsell, *Lieut.-Col. R.E.* (Retired).
George Craig Saunders Pauling.
Frederick Arthur Robinson.
George James Stevens.

(*Paper No. 2902.*)

"The Influence of Carbon on Iron."

By JOHN OLIVER ARNOLD, F.C.S.

THE influence of carbon on iron is a question of great international interest, and during the last twenty-five years it has been the subject of research by a large number of metallurgists and physicists in America, England, France, Germany, Russia and Sweden. The result of these researches has been the accumulation of a vast store of valuable facts accompanied by a number of more or less probable theories. Nevertheless, there has not yet been enunciated a theory of the fundamental laws governing the chemical physics of steel, which has commanded general acceptance.

HISTORICAL.

To attempt even to summarize the whole of the work that has been accomplished in connection with this matter would inordinately lengthen the Paper. Moreover, the excellent bibliographies by Mr. F. Osmond [1] and by Mr. R. A. Hadfield,[2] form excellent indexes to the localities and the sequence of these researches. The names quoted therein will be found to include those of such distinguished investigators as Abel, Åkerman, Roberts-Austen, Barrett, Barus, Brinell, Le Chatelier, Gore, Hadfield, Howe, Ledebur, Martens, Müller, Osmond, Sorby, Chernoff, Wedding and Werth. In intimate connection with the subject of this Paper, the work of Messrs. Brinell and Chernoff, on the importance of what are known as the critical points in the practical manipulation of steel, are deserving of special attention.

Metallurgists are particularly indebted to Messrs. Abel and Müller, by whom was independently discovered the fact that the carbon in unhardened steels exists chiefly as the definite carbide Fe_3C. Undoubtedly, however, the researches which have produced the most important results are: first, the microscopic

[1] Journal of the Iron and Steel Institute, 1890, No. 1, p. 69 *et seq.*

[2] *Ibid*, 1894, No. 1, p. 177 *et seq.*

investigations of Dr. Sorby, who attacked the problem by regarding steel as a crystalline rock; secondly, the continuation to a definite issue by Osmond of the work commenced by Messrs. Barrett, Brinell, Gore and Chernoff, on the critical points of steel. Unfortunately for metallurgists concerned practically with steel, Messrs. Osmond and Sorby deduced from the results of their researches very different conclusions. Dr. Sorby expressed the opinion that cold, unhardened steel, rich in carbon, consisted of alternate laminæ of soft iron and an intensely hard compound of iron and carbon; but that on heating such steel the hard plates[1] formed with the iron another compound stable only at high temperatures, which on cooling again split up into alternating plates of iron, and a hard compound which he very happily named "the pearly constituent" from its property of presenting interference colours. Upon the structure of hardened steels, Dr. Sorby was unable to express any definite opinion.

Mr. Osmond, from the results of his own investigations and those he carried out jointly with Mr. Werth, came to the conclusion (strongly supported by Prof. Roberts-Austen) that although the carbon in cold, unhardened steel existed mainly as Fe_3C, yet on heating such steel to a temperature slightly exceeding 700° C., the iron and carbon dissociated, absorbing heat at the critical point Ac 1; and that the atoms of carbon were then in a free state, not as graphite, but merely dissolved in the mass of iron atoms. The latter, however, underwent an allotropic change resulting in the production of molecules of iron in which are presumably condensed a larger number of atoms than those present in iron in its ordinary condition. This alleged new allotropic modification, which Mr. Osmond named β iron, was stated to possess extreme hardness; and it could be rendered stable at the ordinary temperature when the metal contained sufficient dissolved free carbon and was suddenly quenched in water from a full red heat. Mr. Osmond and Prof. Roberts-Austen therefore stated that the adamantine nature of hardened steel was due mainly to β iron; the hardness being, however, augmented by the presence of the dissolved atoms of free carbon which acted as a frictional resistance. How or why the atoms or molecules of a dissolved element in a free state should, without chemical combination, maintain an atomic condensation in the

[1] These have been shown by the Author and Mr. A. A. Read (Journal of the Chemical Society, vol. lxv. 1894, p. 788), to consist of pure crystallised Fe_3C, which conclusion will presently be confirmed by additional data.

molecules of the solvent, is a matter upon which the originators of the theory have not been very explicit. The Author is therefore unable to offer their explanation or to formulate even diagramatically any possible conception of such an influence. However, it is evident, and is indeed admitted by Mr. Osmond, that the foundation-stone of the β iron theory is an assumption that the carbon present in steel above 700° C., or in the quenched metal is, in the free state, dissolved in and not combined with the iron. If it can be proved that at high temperatures the carbon still remains in combination with the iron, the foundation of the β iron theory will be destroyed and its superstructure must naturally collapse.

The present Paper embodies the results of research, extending over five years, undertaken to decide this vital point. The conclusions arrived at seem to explain satisfactorily the multiple and mysterious effects of carbon upon iron under varying thermal conditions, not only on theoretical grounds, but also because the various deductions accord well with every-day experience of steel metallurgy. The Author is fully aware that his enunciation and proofs must very properly be submitted to critical scrutiny before they are entitled to general acceptance. The following question will probably suggest itself: If the long line of able observers quoted were unable to solve so difficult a problem, is it likely that one experimenter could do so in a single research? The Author's reply to such a question is that in former researches the observations made have been confined to one or two lines of investigation upon materials of unsystematic composition, and that the problem under such conditions was incapable of complete solution.

The plan of investigation adopted by the Author included—

1. The preparation of a series of absolutely sound 50-lb. ingots, 3 inches square, of iron and carbon steels in which the last-named element varied; being in the ingots when hammered and rolled down to bars $1\frac{1}{16}$ inch in diameter, between 0·1 per cent. and 1·5 per cent.—thus ranging from the mildest to the hardest steels usual in practice, and containing the smallest possible percentage of impurities.

2. A complete chemical analysis of each bar.

3. Differential carbon determinations of the forms in which the element exists in normal annealed and hardened steels.

4. The mechanical properties of the normal hardened and annealed steels under compression, and of the normal and annealed steels under tensile stress.

5. The micro-structures of the various steels in the normal annealed and hardened conditions.

6. The measurement, by Mr. Osmond's method, of the heat absorbed on heating and of that evolved on cooling by the various steels at the carbon change point $A_R 1$, and the determination of the heat evolved on tempering hardened steel.

7. The comparative magnetic properties of the hardened and tempered steels.

8. The determination of the correlative points (if any) registered by the different methods of observation.

CHEMICAL.

The analyses of the steel were made on drillings from the centre of each bar. The carbon was determined with every precaution by combustion. The results are tabulated in Table I.

TABLE I.—ANALYSES OF SPECIMENS OF STEEL.

Steel No.	Total Carbon.	Graphite in annealed sample.	Silicon.	Manganese.	Phosphorus.	Sulphur.	Aluminium.	Iron (H and N) by difference.	Total impurities.
1	0·08	..	0·03	0·02	0·02	0·03	0·02	99·80	0·12
1½[1]	0·21	..	0·05	0·05	0·02	0·03	0·02	99·62	0·17
2	0·38	..	0·03	0·08	0·02	0·02	0·03	99·44	0·18
3	0·59	..	0·07	0·10	0·02	0·02	0·03	99·17	0·24
3½[2]	0·74	..	0·05	0·01	0·02	0·02	0·02	99·14	0·12
4	0·89[3]	..	0·03	0·09	0·02	0·02	0·03	98·92	0·19
5	1·20	0·28	0·07	0·15	0·02	0·02	0·03	98·51	0·29
6	1·47	1·14	0·08	0·13	0·02	0·01	0·04	98·25	0·28

For the determination of the forms in which the carbon was liberated from three typical steels in the normal annealed and hardened conditions, the Author is indebted to Mr. A. A. Read, lecturer on metallurgy at University College, Cardiff, whose skill in this difficult branch of analysis entitles the results to unqualified acceptance. The determinations were made in the manner described in the Paper[4] already referred to, except that, in this series, the current was obtained from storage-cells. The results are detailed in Table II.

[1] Interpolated early in research.
[2] Interpolated (late in research) for physical curves.
[3] Average of five combustions.
[4] Journal of the Chemical Society, vol. lxv. 1894, p. 788.

TABLE II.—DIFFERENTIAL DETERMINATIONS OF THE FORMS OF CARBON IN NORMAL, ANNEALED AND HARDENED STEELS.

Steel No.		Weight Taken.	Weight of Residue Dried at 100° C.	Appearance of Dry Residue.	Analysis of Residue (Dry). Iron.	Carbon.	Water.[1]	Carbon obtained as Fe_3C.	Carbon obtained as Hydrate.	Carbon Lost as Hydro-carbons.	Total Carbon.
		Grams.	Grams.								Per cent.
2	Normal	3·283	0·1481	Dark grey powder	89·65	6·87	3·48	0·31	..	0·07	0·38
	Annealed	3·345	0·1485	Minute silvery plates	91·58	7·18	1·24	0·32	..	0·06	0·38
	Hardened	3·257	0·0325	Brown-black powder	29·54	24·19	46·27	0·02	0·22	0·14	0·38
4	Normal	6·641	0·8665	Dark grey powder	89·87	6·47	3·66	0·85	..	0·04	0·89
	Annealed	6·922	0·8175	Minute silvery plates	92·05	6·97	0·97	0·83	..	0·06	0·89
	Hardened	5·635	0·1055	Brown-black powder	37·15	35·55	27·30	0·05	0·61	0·23	0·89
6	Normal	5·306	0·421	Dark grey powder	76·01	18·20	5·79	0·43	1·02[2]	0·02	1·47
	Annealed	5·880	0·1085	Metallic-looking black powder	22·86	73·09	4·05	0·03	0·18[3]	0·12	1·47
	Hardened	5·096	0·140	Brown-black powder	28·57	42·71	28·72	0·06	1·17	0·24	1·47

It will be seen that in steel No. 2 from both the normal and the annealed sample about 85 per cent. of the total carbon was obtained in the form of the definite carbide Fe_3C. From steel No. 4 about 95 per cent. was so obtained. In the annealed samples the carbide had crystallized in silvery plates, whilst in the normal samples it was diffused as small ill-defined plates and granules. On reference to the respective micro-sections, Figs. 6, 12, 8, and 16, Plate 4, the appearance of the two forms of carbide *in situ* will be seen, and it will be hereafter shown that the dark areas in the normal sections contain about 13 per cent. of diffused Fe_3C.

The losses of carbon, viz., respectively about 15 per cent. and 5 per cent. of the total, are in the Author's opinion due, not to decomposition, but to the presence of a small quantity of a readily decomposed intermediate carbide to which the Author tentatively assigns the hypothetical formula $Fe_{10}C$. This compound will be

[1] Taken by difference and including a small quantity of silica.
[2] Free carbon.
[3] Also 1·14 per cent. of graphite, or "temper-carbon."

further mentioned in the microscopical and physical sections. In all the hardened steels, it will be noted that only about 5 per cent. of the total carbon was obtained as Fe_3C. There were considerable losses in the form of hydro-carbons, but the main portion of the carbon was obtained in the form of hydrate. The Author will presently demonstrate that this hydrate results from the decomposition of a chemically unstable sub-carbide of iron.

In the case of No. 6 steel annealed, an inspection of its analysis and micro-section, Fig. 18, Plate 4, will at once explain the results obtained. The figures recorded for No. 6 steel in the normal condition are unique in the series. Less than 1½ per cent. of the total carbon has been lost, but only about 29 per cent. has been obtained as Fe_3C and nearly 70 per cent. as free carbon. On reference to the micro-section, Fig. 10, Plate 4, it will be seen that it contains not only the diffused carbide usual in normal steels but also a large number of membranes and striæ of crystallized Fe_3C. There is little doubt that these two constituents set up with the dilute acid a secondary galvanic action, the crystallized compound acting as the cathode, whilst the diffused carbide decomposes with the formation of ferrous chloride and free carbon.

MECHANICAL.

Definitions.—Unless otherwise stated, the following are the definitions throughout this Paper for the terms "normal," "annealed" and "hardened" steels.

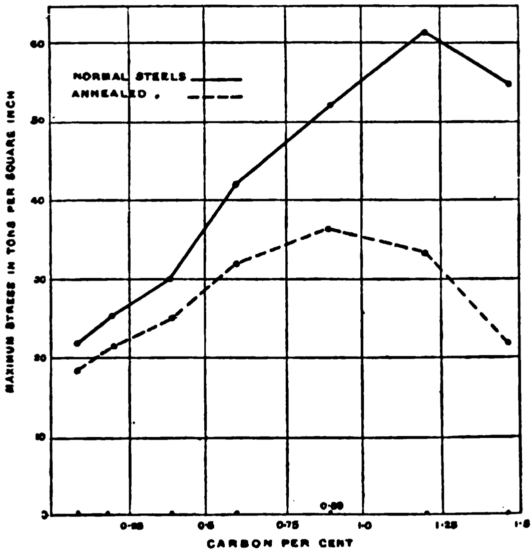
"Normal steels" as received from the rolls have been heated in a muffle furnace to a temperature of about 1000° C., being then allowed to cool in air.

"Annealed steels" have been heated in quick-lime in a covered cast-iron box to a temperature of about 1000° C. for 72 hours, being then allowed to cool in the luted furnace during 100 hours.

"Hardened steels" have been heated in a closed muffle to a temperature of about 1000° C., and afterwards rapidly quenched in a large tank of cold water.

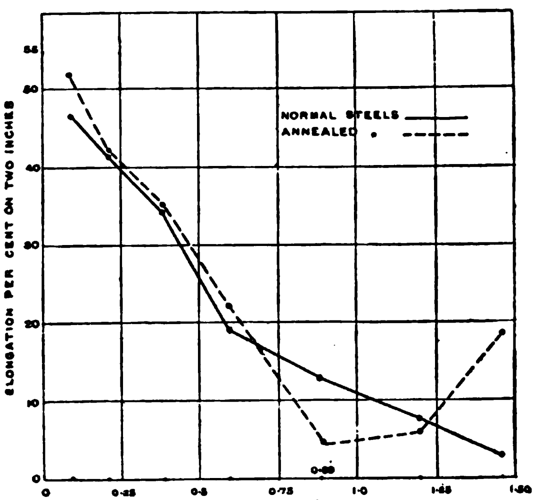
Tensile Tests.—The results of the tensile tests of the normal and annealed steels are tabulated in Tables III and IV respectively. The influence of carbon on the tenacity as evidenced by the maximum stress, and on the ductility as measured by the percentage of elongation, are graphically shown as curves in *Figs. 1* and *2* respectively.

Fig. 1.



CURVES SHOWING THE INFLUENCE OF CARBON ON THE TENACITY OF IRON IN NORMAL AND ANNEALED STEELS.

Fig. 2.



CURVES SHOWING THE INFLUENCE OF CARBON ON THE DUCTILITY OF IRON IN NORMAL AND ANNEALED STEELS.

TABLE III.—TENSILE TESTS ON NORMAL BARS.

Test-pieces 2 inches parallel by 0·564 inch diameter.

Steel No.	Carbon.	Elastic Limit.	Maximum Stress.	Elongation.	Reduction of Area.	Fracture.
	Per cent.	Tons per sq. inch.	Tons per sq. inch.	Per cent.	Per cent.	
1 [1]	0·08	12·19	21·39	46·6	74·8	Grey granular silky edges.
1½ [1]	0·21	17·08	25·39	42·1	67·8	" " " "
2 [1]	0·38	17·95	29·94	34·5	56·3	" " " "
3 [1]	0·59	19·82	42·82	19·9	22·7	Crystalline.
4 [2]	0·89	24·80	52·40	13·0	15·4	"
5 [2]	1·20	35·72	61·65	8·0	7·8	"
6 [2]	1·47	32·27	55·71	2·8	3·3	"

TABLE IV.—TENSILE TESTS ON ANNEALED BARS.

Test-pieces 2 inches parallel by 0·564 inch diameter.

Steel No.	Carbon.	Elastic Limit.	Maximum Stress.	Elongation.	Reduction of Area.	Fracture.
	Per cent.	Tons per sq. inch.	Tons per sq. inch.	Per cent.	Per cent.	
1	0·08	8·82	18·34	52·7	76·7	Grey granular silky edges.
1½	0·21	9·02	21·25	42·3	65·7	" " " "
2	0·38	9·55	25·02	35·0	50·6	" " " "
3	0·59	11·36	31·87	22·0	23·3	Crystalline.
4	0·89	16·81	36·69	4·5	4·2	"
5	1·20	16·19	32·87	6·0	4·9	"
6	1·47	10·08	22·33	19·0	17·7	Dark grey fibrous.

It will be seen that the tenacity of the normal steels reaches a maximum at 1·2 per cent. of carbon, a point coincident with the complete development of a remarkable change in the microscopical structure, viz., the complete environment of the crystals of steel with thin membranes of crystallized Fe_3C. A decisive increase (at 1·47 per cent. of carbon) in the number and thickness of these hard membranes is followed by a falling off in the maximum stress (Figs. 9 and 10, Plate 4).

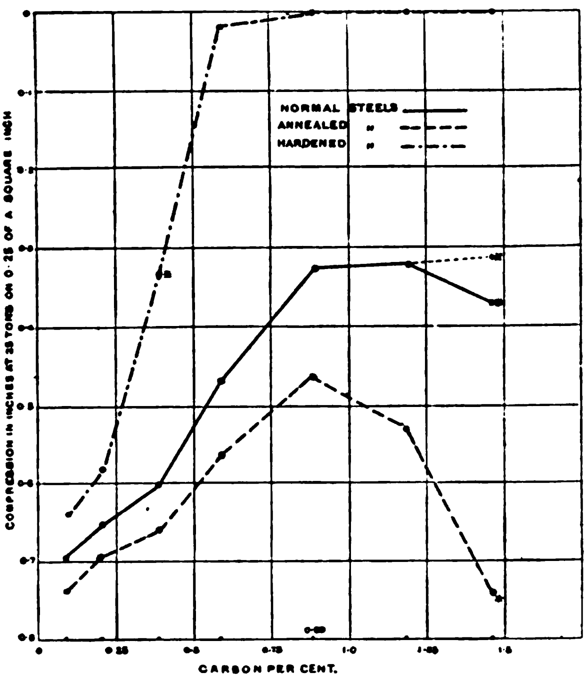
In the annealed curve, up to 0·89 per cent. of carbon, its distance from the normal curve measures the diminution of tenacity due to the change of diffused into crystallized Fe_3C. The maximum tenacity in the annealed series is reached at No. 4 steel; the curve then dips owing to separation of graphite, Figs. 17 to 19, Plate 4.

[1] Mean of two tests. [2] Average of three tests.

The curves of ductility are worthy of close attention. It will be noted that up to 0·65 per cent. of carbon the annealed samples are slightly more ductile than the normal bars, but that between 0·65 per cent. and 0·9 per cent. the ductility of the annealed is actually less than that of the normal metals. Inspection of Fig. 16, Plate 4, will show that the continuity of the structure of steel No. 4 is broken in every direction by the presence of innumerable hard plates of crystallized Fe_3C. The upward movement of the curve above 0·89 per cent. is again due to a separation of carbide of iron into crystals of free iron and particles of graphite.

Compression Tests.—The results of the compression tests on the

Fig. 3.



* Approximate position obtained by projecting the curve from the last compression-point registered (at 20 tons) previous to rupture.

CURVES SHOWING COMPARATIVE MOLECULAR FLOW OF STEELS UNDER COMPRESSION.

normal, annealed and hardened steels are tabulated in Table V, and are plotted as curves in *Fig. 3*.

TABLE V.—COMPRESSION TESTS.

The diameter of the test-pieces was 0·564 inch, and their length was 1·126 inch = 2 diameters nearly.

Pressure.	Condition of Steel.	No. of Specimen.						
		1	1½	2	3	4	5	6
Tons.		Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
5	Normal .	0·072	0·037	0·032	0·011	0·005	0·007	0·010
	Annealed	0·055	0·069	0·052	0·030	0·022	0·025	0·055
	Hardened	0·018	0·012	0·010	0·000	0·000	0·000	0·000
10	Normal .	0·297	0·216	0·159	0·056	0·018	0·012	0·020
	Annealed	0·328	0·306	0·245	0·124	0·065	0·078	0·285
	Hardened	0·180	0·059	0·022	0·000	0·000	0·000	0·000
15	Normal .	0·507	0·432	0·369	0·200	0·058	0·032	0·037
	Annealed	0·540	0·529	0·470	0·330	0·208	0·260	0·520
	Hardened	0·405	0·246	0·042	0·001	0·000	0·000	0·000
	Normal .	0·630	0·585	0·527	0·370	0·190	0·100	0·123
	Annealed	0·660	0·666	0·627	0·508	0·382	0·438	0·680
20	Hardened	0·567	0·533	0·164	0·002	0·000	0·000	0·000
	Normal .	0·697	0·648	0·601	0·474	0·326	0·320	broken
25	Annealed	0·726	0·693	0·658	0·565	0·466	0·521	broken
	Hardened	0·632	0·571	broken	0·006[1]	0·000[2]	0·000	0·000
Combined carbon per cent.	Normal . Hardened	0·08	0·21	0·38	0·59	0·89	1·20	1·47[3]
	Annealed	0·08	0·21	0·38	0·59	0·89	0·92[4]	0·33[5]

The Author holds the view strongly that compression tests furnish by far the best mechanical means of measuring the molecular rigidity of metals because in this method the interfering effects of variations in the inter-crystalline cohesion are reduced to a minimum. The curve of the normal steels shows a falling off in the flow till the iron contains 0·89 per cent. carbon, when it is practically stationary to 1·5 per cent. of carbon, the Author having reason to think that the dotted line to the cross marks the true flow, the observed downward break being due to an undesired separation of graphite accompanied by the introduction of particles

[1] Hardened steel No. 3 registered a compression of 0·010 inch at 30 tons; between 30 tons and 35 tons it explosively disintegrated.

[2] Hardened steel No. 4 registered a compression of 0·000 at 30 tons; further tested without practical compression, to 200 tons per square inch, when the machine compression-plates broke.

[3] Some graphite present in the specimen of normal steel.

[4] 0·28 per cent. of graphite present.

[5] 1·14 " " "

of soft iron into the structure. The annealed curve up 0·89 per cent. of carbon measures graphically the effect on the flow of a change from diffused to crystallized Fe_3C. After 0·89 per cent. of carbon the curve breaks downwards till at 1·47 per cent. total carbon (the bulk of which, however, exists as graphite), the flow is actually greater than that of the nearly pure iron, a fact probably due to the loosely deposited graphite crystals being themselves capable of some compression (see various micro-sections). The curve of the hardened steels is most remarkable and important. It will be noted that the molecular flow absolutely ceases at 0·89 per cent. of carbon, whilst the curve from 0·2 per cent. to 0·6 per cent. is very steep.

It is shown subsequently that the metals contain an amount of intensely hard sub-carbide of iron proportional to the carbon, and at 0·89 per cent. constituting 100 per cent. of the metal. The remarkable decrease in the flow between the points 0·38 per cent. and 0·59 per cent. of carbon is readily explained by an inspection of the micro-sections Figs. 6, 7, and 21, Plate 4, and of Table IX. It will be seen that the theoretical free iron in No. 3 steel is about 33 per cent., that in No 2 steel being about 58 per cent. Therefore No. 2 steel may be mechanically represented as somewhat equivalent to a mass of lead in which are suspended fragments of glass, whilst No. 3 steel resembles a mass of glass in which are suspended fragments of lead. It is obvious that the fragments of lead would not greatly diminish the rigidity of the mass of glass in which they were suspended. Nor would the fragments of glass proportionally impair the capability of the mass of lead to flow under the influence of compression.

Throughout the mechanical tests there will have been noticed the tendency of most of the curves to break at the point 0·89 per cent. of the carbon co-ordinate. These breaks, it will be seen later on, are merely mechanical reflections of breaks in the micro-structure, in the thermal and in the magnetic curves, all due to a common cause, and constituting foundation-stones upon which to build the true theory underlying the chemical-physics of steel.

MICROSCOPICAL.

The study of this very important branch of steel metallurgy, initiated thirty years ago by Dr. Sorby, has attracted considerable attention on the Continent. In Germany, particularly, great strides have been made in its development under the superintendence of Professor Martens. By English metallurgists

it has been until quite recently almost neglected, or condemned with faint praise. The mechanical difficulties of preparing a perfect section and of delicately etching the surface, so as to reveal its true structure when examined with high powers as an opaque object, are considerable. The Author has only overcome these obstacles after some years of laborious experiment, for which, however, he has been amply repaid by the discovery that the laws determining the structure of iron containing various percentages of carbon, are fixed and concordant for given physical conditions; in fact, from puzzling chaos, he has been enabled to evolve order of a most interesting character, supplying, moreover, the key to the position he was attacking.

The Constituents of Iron and Carbon Steels.

Pure Iron.—Perfectly pure iron is never met with in commercial masses, but in Swedish Lancashire hearth-rolled bars, containing in their average analysis 99·8 per cent. of iron, groups of almost chemically pure crystals of the metal may be met with. They are readily distinguished by their well-defined facets and angles, and by the fact that they remain bright and smooth even after prolonged attacks by the excessively dilute nitric acid used for etching, which merely penetrates and makes visible the fine junction-lines of the crystals. In *Fig. 4* is shown a micro-metric reproduction of crystals of pure iron viewed by direct illumination and magnified 600 diameters. Their geometric form agrees most nearly with that produced by interfering cubes and octahedra with dominant cubic faces.[1] It is, however, unusual to meet with such well-defined and geometrical crystals as those figured, because of the distortion-stresses taking place in the metal during cooling after cystallization, which phenomenon the Author's experiments[2] indicate as occurring at a moderate red-heat, the formation commencing at 750° C. and being completed at 720° C.

Slightly Impure Iron.—In wrought-iron and in mild steels the free iron crystals are often somewhat contaminated with a little residual carbon, which causes them during the process of etching to assume a pale brown tint and a rough surface. The amount of carbon so involved is very small, seldom exceeding 0·05 per cent., and its mechanical influence is insensible. The Author,

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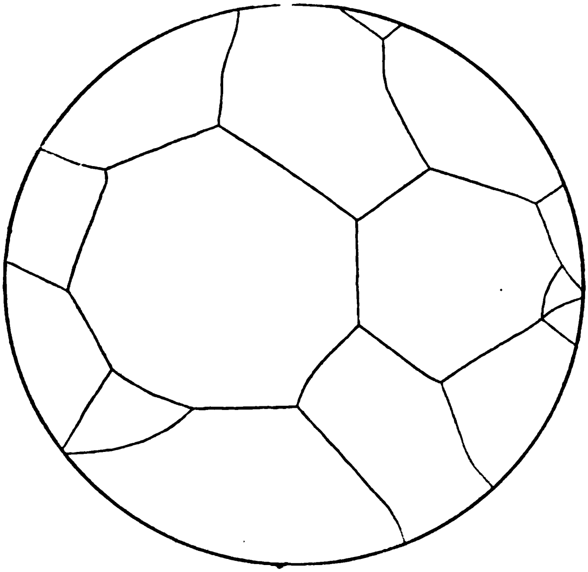
[1] In a recent communication to the Royal Society, Mr. Thomas Andrews, F.R.S., has shown that in heavy slowly-cooling masses of wrought-iron the large primary crystals often split into numerous secondary cubes.

[2] Journal of the Iron and Steel Institute, 1894, No. 1, p. 132 *et seq.*

therefore, will not at present further discriminate between the two kinds of iron crystals, though the exact nature of the carbide existing in the tinted crystals has some molecular interest in connection with Osmond's point A_R 3. When very mild steels are submitted to prolonged heating in a vacuum at a temperature of 1,400° C. and are then cooled in air, the micro-structure of the steels undergoes a distinct change, in which the knots of normal carbide of iron disappear and a large increase takes place in the number of tinted crystals. These facts are correlated thermally

Fig. 4.



Magnified 600 diameters.

PURE IRON CRYSTALS (Swedish Lancashire hearth-rolled bar-iron).

with a large permanent increase in the heat evolved at A_R 3 on cooling, the carbon change point almost disappearing at A_R 1 and its position at A_R 3 being raised about 10° C. These results are consistent with the theory that there may exist traces of a carbide of iron intermediate in formula between the normal carbide Fe_3C, and the all-important sub-carbide to be presently described.

Diffused Normal Carbide.[1]—These areas, when the polished

[1] These correspond with the "amorphous iron" of Dr. Müller.

section is immersed in very dilute nitric acid, are at once partly covered with a dark brown film of carbonaceous colouring matter, the latter thus constituting an invaluable automatic staining medium. The dark areas, as will be shown subsequently, consist of iron containing about 13 per cent. of normal carbide Fe_3C diffused through its substance in the form of small ill-defined plates and granules. They also mark the preliminary stage of formation of the "pearly constituent."

The Pearly Constituent.—This constituent is best developed in annealed steels and presents the well-known hard and soft laminæ discovered by Dr. Sorby. It has already been shown that the hard laminæ are crystals of Fe_3C. The soft interspaces are nearly pure iron. The parallel carbide plates may be wavy or straight, and they differ much in thickness and their distances apart. When the iron interspaces are very wide, the carbide plates are distinctly seen to be in relief, the fibres of the polishing blocks having excavated the soft iron. Sections containing much "pearly constituent" present on etching a beautiful play of pearly or opaline interference colours, which, if the etching is very light, are permanent.

Crystallized Normal Carbide (Fe_3C).—This substance, exclusive of its occurrence in the pearly constituent, may gather into large sectional rivers or into isolated masses. It requires then an experienced eye to distinguish it from perfectly pure iron, but, as a rule, the fact that it is always in relief and its brilliant silvery surface serve to identify it.

Graphite.—This is Ledebur's "temper carbon." For English-speaking metallurgists, a more unfortunate name could hardly have been chosen. "Annealing carbon" would have been better, but to make its nature quite clear the Author will throughout this Paper employ the name graphite, as expressing for all practical purposes what the substance is. In steel it occurs in the form of dark rounded dots (or more rarely in short worm-like masses) well defined against a back-ground of pale iron.

Sub-carbide of Iron (or β iron).—But one more constituent remains to be described, and this, if Mr. Osmond's theory be true, must be β iron charged with dissolved carbon. On lightly etching a polished section consisting mainly of this compound, it retains its polish but assumes a "black-leaded" appearance, due to a very faint coating of dark carbonaceous matter. It seems homogeneous and apparently non-crystalline, but probably consists of minute crystals, the junction-lines of which are beyond the range of microscopic vision or are obscured by the faint carbonaceous deposit. When

deeply etched, this substance becomes covered with a velvet-black deposit which may be removed by the finger, staining the latter. The body just described is found only in hardened or hardened-and-tempered steel. The Author will presently produce what seems to him conclusive microscopical thermal and magnetic evidence that this substance is not an allotropic modification of iron, but a definite though remarkably attenuated and unstable carbide of iron of intense hardness and corresponding with the formula $Fe_{24}C$.

DETAILS OF MICROSCOPIC OBSERVATIONS.[1]

The structures illustrated, Plate 4, were all drawn from the microscope, when necessary a micrometer being used on correspondingly graduated circles 28 inches in diameter. The drawings were then reduced by photography to the diameter of the microscopic field. The labour involved in carrying out this process was great, but the results depict the structures with an accuracy unattainable by direct photography. Direct illumination was employed throughout.

Normal Steel No. 1 (Carbon 0·08 per cent.). Fig. 5, Plate 4.—The structure consists of irregular crystals of iron, amongst which are sparingly distributed small dark knots of the diffused normal carbide areas. On comparing this section with that of the pure iron, it will be seen that the presence of even 0·08 per cent. of carbon is at once decisively revealed by the microscope.

Annealed Steel No. 1 (Carbon 0·08 per cent.). Fig. 11, Plate 4.—The effect of annealing had been to produce a distinct increase in the size and geometry of the iron crystals, and to gather the Fe_3C, diffused through the dark normal areas, into isolated patches of coarse pearly constituent surrounded by thick sectional meshes of crystallized Fe_3C.

Normal and Annealed Steels, No. 1½ (Carbon 0·21 per cent.).—These sections were in all respects intermediate between those of steels Nos. 1 and 2. It was not therefore deemed necessary to illustrate them.

Normal Steel, No. 2 (Carbon 0·38 per cent.). Fig. 6, Plate 4.—This section consists of a mixture of crystals of iron, and large irregular dark areas of diffused normal carbide, the latter occupying on an average nearly half the field.

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[1] In examining the engravings of the sections, a lens will be found useful for some of the finer structures.

Annealed Steel, No. 2 (Carbon 0·38 per cent.). Fig. 12, Plate 4.—On annealing, the iron crystals have become larger and more definite, whilst the dark areas have aggregated; and on cooling the components have segregated, forming striæ of crystallized Fe_3C, divided by spaces of iron. The groups of striæ are often partly surrounded by sectional meshes of Fe_3C, a few isolated striæ of which compound may be sometimes observed between the junctions of the iron crystals.

Annealed Steel, No. 2 (Carbon 0·38 per cent.). Fig. 13, Plate 4.—This section gives a general view of the structure over a comparatively large area. It forms a beautiful microscopic object resembling an irregular mosaic pavement composed of white crystals of iron and large irregular patches of the pearly constituent, showing splendid interference colours. Of course a magnification of 100 times a linear dimension is insufficient to resolve the striæ of the pearly constituent.

THE GENERAL INFLUENCE OF ANNEALING ON MILD CAST-STEEL.

As No. 2 steel contains about the same percentage of carbon as that contained in high-class castings, it will be well to discuss in connection with it the general principles underlying the operation of flame annealing. The surface oxidation of carbon resulting from this process will be neglected, as its effect is comparatively small, and only the action of annealing on the main portion of the steel which is unaltered in ultimate chemical composition will be considered.

The ideas prevalent among metallurgists on this subject are often very erroneous. It has been stated, both in text-books and in practical papers, that the action of annealing is to produce smaller crystals. As a matter of fact, the crystals of an annealed steel are always larger than those existing in the metal before annealing. The idea of smaller crystals has doubtless arisen from a confusion of effect with cause. After annealing, the crystals of the fractured metal do appear to the eye smaller than those in the original material; but the reason for this is found in the fact that during the process of rupture they elongate, what are really seen being the ends of the ductile, and consequently drawn out, crystals, and not (as in the case of the comparatively brittle unannealed metal) the originally existing facets. Also, frequently, what are regarded as crystals in the fracture of a brittle unannealed steel casting are really groups containing many crystals, originally bounded by lines of great inter-crystalline

weakness, along which rupture has naturally taken place. As a rule the fracture of steel bears little or no relation to its true micro-structure. It has also been stated that on annealing, the iron and the pearly constituent become more intimately mixed; for which Dr. Sorby has been quoted as the authority. Dr. Sorby's general conclusion on this matter was accurate, and was exactly opposite to that attributed to him. The mistake seems to have arisen from an imperfect knowledge of the meaning of the word "segregate." The true action of annealing on a moderately mild steel, containing, say, 0·35 per cent. of carbon, is as follows:—1. The comparatively small and distorted crystals of the original metal become larger and more geometrical in form (they are therefore freer from internal stresses), and the inter-crystalline cohesion, if originally weak, is much strengthened.[1] 2. The carbonized areas existing in the unannealed steel, chiefly in irregular elongated masses, gather together during the slow cooling into rounded or harp-shaped areas, in which form they favour the continuity of the iron crystals to a much greater extent than the original arrangement. 3. The rounded or harp-shaped areas, into which are concentrated the normal carbide of iron split up during the slow cooling into plates of crystallized Fe_3C, separated by large interspaces of iron, hence the latter become dove-tailed into the main body of the iron crystals. This continuity, however, is not perfect, being frequently broken by the sectional meshes partly environing the laminated areas. Thus long lines formed by a juxtaposition of two distinct constituents are broken up, and the iron becomes almost continuous throughout. In fact, the carbon is concentrated into small plates suspended in the iron, constituting only about 5 per cent. of the total metal instead of being distributed in large more or less continuous areas forming about 40 per cent. of the mass. The foregoing statements are common to the cases of forged steel, as well as of the metal as cast, but they apply with peculiar force to the last-mentioned material, in which the inter-crystalline cohesion is usually weak; which is not often the case in forged steels, because the work put upon the material has already repaired the faulty crystal-junctions in a manner analogous to the influence exercised by annealing.[2]

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[1] Perfect inter-crystalline cohesion is synonymous with that hitherto mysterious essence known to the practical man as "body."

[2] The question of the influence of annealing on the various types of steel castings is of sufficient importance to warrant its special consideration in a separate Paper, for which the Author has for some time past been collecting data, some of which are of a startling nature.

In order to render the above facts more clear, half fields of No. 2 steel in the annealed and normal conditions are exemplified in Fig. 14, Plate 4. The sections from which these were drawn were very lightly etched so as to bring out only the carbonised constituents without developing the lines marking the inter-crystalline junctions. The several sections of No. 2 steel show clearly that existing views as to steel being built up of a series of cemented cells, are erroneous. As already stated, streaks of carbide cement may now and then be seen between the junctions of the iron crystals, but such constitute incidents and not a principle. As a matter of fact, if the facets of the iron crystals were really united by Fe_3C cement, a mild steel would be easily fractured by a blow from a heavy sledge-hammer. The Author, from the results of many experiments, confidently makes the following statement:—

If the cohesive force acting between the facets of crystals is from any chemical, thermal or mechanical cause seriously weakened, the metal will appear to be very brittle, owing to rupture under the influence of a sudden shock occurring along the weak junction lines, in spite of the fact that the molecular cohesion may be perfect and the individual crystals ductile.[1]

From the foregoing statement, it will be obvious that a metal may be soft to the drill or under compression, and yet brittle under impact, exhibiting little or no ductility in tension. It is also clear that in such a case chemical analysis is useless. The Author, however, is not yet prepared to state the exact means by which inter-crystalline weakness may be measured by the microscope, but he is hopeful that in the near future such measurements may be possible. That in nearly pure iron the inter-crystalline cohesion and the molecular cohesion may be equal, is proved by the tensile test of No. 1 steel annealed. This metal is composed of large definite crystals, Fig. 11, Plate 4, yet the elongation was 53 per cent. and the reduction of area at the point of fracture in tension was 77 per cent.

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[1] Purely scientific investigators dealing with the physics of iron, discourse freely on molecules and their distances, but they ignore crystals and their comparatively huge interspaces. This is a grave error; *e.g.*, there is little doubt that magnetic properties are much influenced by the dimensions of the crystals into which the molecules are grouped. The Author emphatically reiterates that deductions explaining observed mechanical facts on the basis of allotropic changes in the molecular architectures of metals are valueless, unless the effects due to crystalline architecture have been previously determined and allowed for. The effects due to the first-mentioned cause are often very small in comparison with the effects produced by inter-crystalline causes.

Normal Steel No. 3 (Carbon 0·59 per cent.). Fig. 7, Plate 4.—In this section the dark normal carbide areas considerably exceed the now isolated and highly distorted crystals of iron.

Annealed Steel No. 3 (Carbon 0·59 per cent.). Fig. 15, Plate 4.—This section confirms, and presents on a larger scale than No. 2 steel annealed, the breaking up of the dark areas into striæ of crystallized Fe_3C separated by interspaces of iron.

Steel No. 3½ (Carbon 0·74 per cent.). Slightly annealed.—This section was in all respects intermediate to the normal sections of steels Nos. 3 and 4, containing fewer iron patches than the former.

Normal Steel No. 4 (Carbon 0·89 per cent.). Fig. 8, Plate 4.—This section presents a feature of vital importance in connection with the theory of steel which the Author will presently enunciate. The entire structure consists of ill-defined crystals forming dark areas of iron containing suspended normal carbide, whilst crystals of iron free from suspended carbide have necessarily altogether disappeared. In other words, iron containing 0·89 per cent. of carbon presents a critical microscopical point which will be hereinafter referred to as the "saturation point;" steels in which the carbon falls below 0·89 per cent. will be termed "unsaturated;" whilst steels containing more than 0·89 per cent. carbon will be distinguished as "super-saturated," for reasons to be presently stated.

Annealed Steel No. 4 (Carbon 0·89 per cent.). Fig 16, Plate 4.—This section consists entirely of crystals of the pearly constituent. The crystallized striæ of Fe_3C are in nearly parallel lines, some straight, others wavy. Small isolated masses of this compound also occur sparingly. The thickness of the plates and of the iron inter-spaces vary considerably. In one area the hard plates are in such relief owing to the wearing away of the broad, soft iron inter-spaces, that they actually cast microscopic shadows as indicated in the figure. The micro-section of this steel, consisting entirely of the pearly constituent, presents to the eye a beautiful play of colours resembling those of mother-of-pearl.

Normal Steel No. 5 (Carbon 1·2 per cent.). Fig. 9, Plate 4.—The main portion of this section is similar to that of normal steel No. 4, but each crystal or group of crystals is surrounded by a sectional mesh of Fe_3C. Isolated striæ of the latter compound also occur. It must be remembered that the strings of carbide which appear sectionally as a coarse and irregular network are in reality, when translated into the solid, more or less perfect investing membranes. This statement is proved by the fact that both transverse and longitudinal sections present the same characteristics.

Annealed Steel No. 5 (Combined Carbon 0·92 per cent., Graphite 0·28 per cent.). Fig. 17, Plate 4.—This section possesses features of special interest. It presents two distinct types of field, which are reproduced in two half-fields. In one will be observed crystals or groups of crystals composed of the pearly constituent enclosed in very large sectional meshes of Fe_3C. These thick membranes have evidently resulted from a confluence, during the slow cooling, of the comparatively small membranes present in the normal steel. In parts of the section from which meshes are absent, there are, however, round almost black patches of graphite, set for the most part in the centres of round patches of bright iron, the remainder of such field being as usual composed of the pearly constituent. It would therefore appear that when the mobilised masses of carbide attain a certain magnitude, they act during the slow cooling like very highly carbonised white pig-iron, dissociating into nearly pure iron and graphite. The temperature at which this separation takes place will be considered in connection with the annealed sample of No. 6 steel.

Normal Steel No. 6 (Carbon 1·47 per cent.). Fig. 10, Plate 4.—In this section the dark background of iron permeated with diffused Fe_3C is much broken up by thick irregular meshes of crystallized Fe_3C. The enclosed crystals also contain large fern-like streaks of the crystallized carbide, the whole constituting a beautiful and striking microscopic object.

Annealed Steel No. 6 (Combined Carbon 0·33 per cent., Graphite 1·14 per cent.). Fig. 18, Plate 4.—In this section about one-third of the area consists of the pearly constituent, the other two-thirds being composed of iron crystals largely spotted with dark round patches, and short worm-like masses of graphite. The latter must have separated below the temperature of the carbon change point Ar_1, which is about 685° C., because the large masses of carbide described in connection with annealed steel No. 5 would in the present case be still greater; and not only have they become totally decomposed, but have evidently also gathered in much of the carbide existing as small plates in the pearly constituent. Hence, as the plates so collected would not have crystallized till the temperature had fallen to about 680° C., it appears certain that the decomposition of the Fe_3C into iron and graphite must take place at or below Ar_1, *i.e.*, at a low red heat. The interesting fact that this dissociation is facilitated by pressure, is proved by the investigations of Mr. B. W. Winder, who found that hard file-steel leaving the rolls at a low red heat, almost invariably contained graphite in large quantities, whilst

similar steel finished at a fair red heat was almost devoid of free carbon.

Annealed Steel No. 6 (Combined Carbon 0·33 per cent., Graphite 1·14 per cent.). Fig. 19, Plate 4.—This section presents a large area of the graphitic metal, showing the crystals of iron, the spots of graphite, and the curiously irregular masses of the pearly constituent in which is contained the 0·33 per cent. of combined carbon present. At 100 diameters the microscope is of course incapable of resolving the striæ of the pearly constituent, which, however, presents a play of gorgeous colours. From the three graphitic sections referred to, it will be seen that super-saturated steels are always very liable to deposit graphite on annealing. Such a phenomenon is seldom or never observed on or below the saturation point.

The foregoing microscopical facts have been known to the Author for about three years, having been ascertained by an examination of another series of steels similar to those now under consideration. But as it was unexpectedly found that the harder steels contained about 0·3 per cent. of manganese, the Author rejected the first series, and made a purer set of steels upon which to commence the research afresh. As the result proved, the comparatively high manganese in the hard steels did not seriously affect the results just described, and the first series now constitute confirmatory evidence which has also been augmented by the examination of many samples of commercial steels.

General Theory Based on the Microscopical Results.

The evidence given by the micro-structure of the steels is thus interpreted by the Author:—

1. The sharply defined localization of the areas containing respectively the diffused normal and crystallized carbides (until the saturation point at 0·89 per cent. of carbon is reached), seems to confirm beyond doubt the accuracy of the general conclusion of Dr. Sorby—that at high temperatures a compound of iron and carbon exists, which on cooling splits into iron and an intensely hard compound very rich in carbon.

2. The fact that the dark carbonized areas of normal steels increase proportionally to the carbon until the saturation point is reached, seems quite incompatible with the theory that at a high temperature the carbon is in a free state in mere solution. Under such conditions, the carbide of iron would be evenly diffused after its deposition *in sitû* on cooling, and would on etching yield an

almost homogeneous microscopic field, darkening in colour as the carbon increased; inasmuch as the stronger the solution at high temperatures, the greater the amount of diffused carbide in the cold metal, and *cæteris paribus*, the thicker the deposit of carbonaceous colouring matter released on the surface of the etched section.

If it be admitted that the dark areas in normal steels and the striated areas in the corresponding annealed metals are mixtures resulting from the decomposition of a compound existing at temperatures above the change point $A_R 1$, it necessarily follows that at the saturation point (at 0·89 per cent. of carbon), the whole mass of the iron is at a full red heat in combination with the carbon, and hence that the percentage of carbon in the saturated steel is also the percentage of carbon in the formula of the compound. Therefore, the compound will contain 0·89 per cent. of carbon, and 99·11 per cent. of iron, corresponding with the formula $Fe_{24}C$, which requires 0·884 per cent. of carbon.

In the case of a super-saturated steel made by gradually adding, say, 1·5 per cent. of carbon to pure iron in a molten state; when the iron has combined with 0·89 per cent. of carbon, it will have been converted into a carbide of formula $Fe_{24}C$; but on adding more carbon, a portion of the sub-carbide will be carbonized to the normal carbide Fe_3C, thus:—

$$Fe_{24}C + 7\,C = 8\,Fe_3C.$$

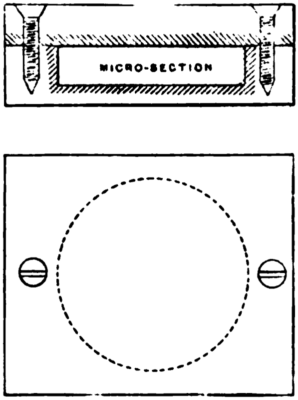
The molten mass then consists of a mixture of the normal carbide with sub-carbide of iron. On cooling, the sub-carbide decomposes into ill-defined crystals of iron permeated with diffused Fe_3C, whilst the surplus normal carbide is thrown off in the form of membranes enveloping the irregular crystals of the mixture resulting from the decomposition of the sub-carbide.

The Structure of Hardened Steels.

To obtain more conclusive microscopic evidence of the accuracy of the theory just enunciated, it was obviously necessary to determine the structure of hardened steel below, on, and above the saturation point. When it is remembered that even the skill of Dr. Sorby was baffled in all his efforts to obtain satisfactory sections of hardened steel, it is not surprising that, although possessing superior appliances, the Author's experiments in this direction were for several years almost fruitless, yielding most puzzling and erratic results. However, comparatively recently, the Author possessed himself of the key to the position, in the fact that it was absolutely necessary to harden the samples from

a nearly white heat, without allowing them to come into contact with either air or water. This was because the decarbonising action of a film of magnetic oxide on the surface of a piece at a full red heat, extended irregularly to such a depth that it was almost impossible in the flint-hard steels to grind off the partially decarbonised surfaces without disturbing the structure or "letting down" the steel. This fatal defect was finally removed by the following simple though somewhat costly plan. Each micro-section was polished and encased air-tight in thin plates of the same steel in the manner indicated in *Fig. 20.* The encased micro-section was then slowly heated to about 1,050° C., and was quenched with the greatest possible rapidity in a large tank of ice-cold water. On drying and removing the casings, the section, although it had been heated during half an hour up to an incipient white heat, was found to be quite bright and absolutely unoxidised on the lower polished face. On lightly etching three typical sections, the following results were obtained.

Fig. 20.



Twice full size.

Hardened Steel No. 2 (Carbon 0·38 per cent.). Fig. 21, Plate 4. —On being etched, the sample assumed a roughish texture and a dull, somewhat dark-grey tint. On lightly removing the grey deposit, the steel was found to consist of two distinct constituents, viz., free iron and an amorphous substance to which the acid had communicated a dark colour. In some fields the iron, and in others the dark constituent, predominated. The Author affirms the latter to be sub-carbide of iron. The section figured represents an average field. The shock of the sudden cooling seems to have dispersed the iron through the dark substance in masses irregular in size and fantastic in shape—many particles, no doubt, being too small for separate microscopic definition.

Hardened Steel No. 4 (Carbon 0·89 per cent.). Fig. 22, Plate 4. —This section, on being very lightly etched, retained its polish but assumed a "black-leaded" appearance. When examined under the microscope the field at first sight presented a brownish-coloured blank, in which no crystalline structure could be detected. A prolonged and careful examination showed that the section really possessed an indefinite granular roughness, but no crystalline junc-

tions could be detected. It is, however, probable that the mass really consists of minute crystals, the boundary lines of which are beyond the reach of microscopic vision or are rendered indefinable by the faint carbonaceous deposit. This is the only practically homogeneous section the Author has ever obtained during many years of close study of the micro-structure of steel and iron.

Hardened Steel No. 6 (Carbon 1·47 per cent.). Fig. 23, Plate 4. —On being etched, this section behaved in every respect like hardened steel No. 4. The groundwork of the section was also found to be identical with the saturated metal, but all over it was spread a network of fine meshes, together with isolated striæ and irregular dots of a substance microscopically corresponding in all respects to Fe_3C.

Thus the micro-structures of the hardened steels seem in accordance with the Author's theory. The unsaturated steel possesses a structure such as might be expected from a suddenly quenched mixture of free iron and sub-carbide of iron.[1] The saturated steel fulfils the necessary theoretical condition of homogeneity; whilst the super-saturated steel decidedly reveals the presence of surplus meshes of normal carbide of iron.

PHYSICAL.

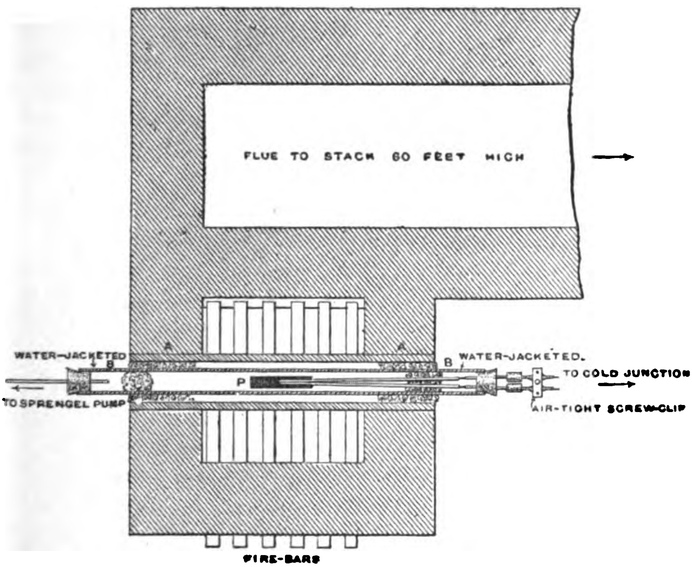
The results obtained in the microscopical section seem *per se* to negative the possibility of the accuracy of the solution theory, and therefore, that of Mr. Osmond's corollary of a hard allotropic modification of iron, whilst every fact observed is in accordance with the sub-carbide theory enunciated by the Author. It, however, remains to submit both theories to the ordeal of examination by the invaluable method of thermal observation inaugurated by Mr. Osmond. If the solution theory be true, and the heat evolved at the carbon change point at Ar 1 on cooling is due to the combination of dissolved free carbon with the iron to form Fe_3C, it follows; and it has indeed been stated by Mr. Osmond, that, within the limits of the hardest steels, the greater the amount of carbon in solution, the larger the amount of Fe_3C formed and consequently the greater the quantity of heat evolved at the critical point Ar 1. On the other hand, if the Author's theory be true, it demands the somewhat startling theoretical condition that the maximum heat at Ar 1 should be evolved from iron containing

[1] The non-homogeneous nature of hardened unsaturated steel is best seen in oil-quenched gun-steel, containing about 0·3 per cent. of carbon, the almost black sub-carbide areas being fantastically emeshed in free iron.

0·884 per cent. of carbon; and that any further addition of carbon should cause a decrease in the amount of heat evolved, because of the displacement of sub-carbide by the normal carbide existing above the saturation point—which normal carbide, owing to the absence of iron to reduce it, remains stable at high temperatures.

Details of Thermal Observations.—A large number of preliminary observations were made and recorded exactly in the manner already fully described.[1] It was, however, found that on repeated heatings, even a partial access of air removed some surface carbon and palpably disturbed the concordance of the results. It was therefore determined to make the observations on polished bars in a vacuum. A sectional plan of the apparatus employed is given in *Fig. 24.* A A

Fig. 24.



Scale, 1¼ inch = 1 foot.

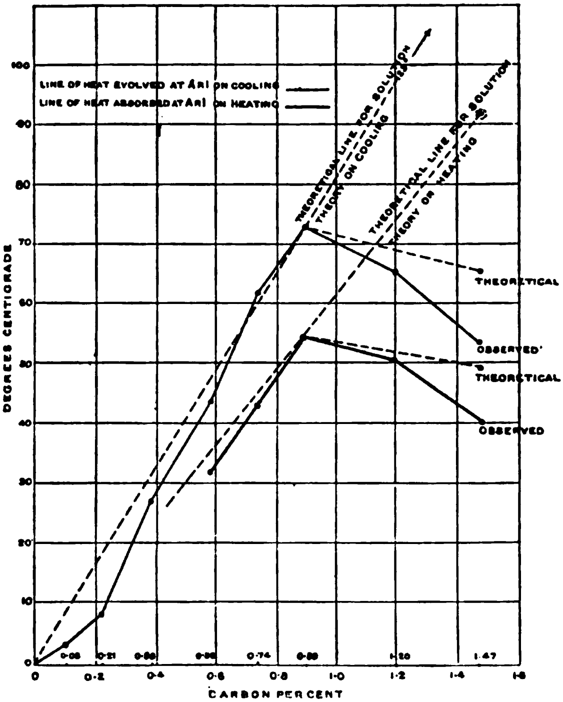
is a fire-clay tube, B B a doubly-glazed porcelain tube connected with a Sprengel pump. P is the test-piece with the thermo-couple in the centre of the mass.[2] The vacuum obtained during the

[1] Journal of the Iron and Steel Institute, No. 1, 1894, pp. 129 *et seq.*

[2] In order to meet an exception taken by Prof. W. C. Roberts-Austen to the Author's method, the recess for the thermo-couple was drilled of a diameter just large enough to admit the twist on the application of a little pressure, so that the bright surfaces of the metals of the couple and steel were in actual contact. No advantage of any kind resulted, however, from this rearrangement.

observations was within 0·3 inch of theory, and the polished bars, after being several times heated to a full red, came out quite bright and unoxidized. It was found necessary on the first heating to go up to 1000° C., and at that temperature to pump out most of the gases contained in the steels. The gases evolved on the first heating usually caused a temporary drop of 4 inches or 5 inches in the vacuum. Very little gas was evolved from the steel below a temperature of 750°, but before 800° was reached the evolution

Fig. 25.



was very brisk. The second and third coolings were made from an initial temperature of 950° C.

This double-tube furnace was found to give remarkably steady chronographic curves of the normal rise and fall of temperature. The mean results of the concordant observations of the heat evolved and absorbed respectively on the second and third coolings and heatings of the bars are recorded in Table VI and are plotted in *Fig. 25.* The heats were calculated by taking the total perturbation from the fair curve (whether occurring as an actual rise or

fall, stay or retard) in seconds and dividing by the mean rate. The result (being the equivalent rise or fall in millimetres) multiplied by 3·1 (the calibration factor) equals the equivalent rise or fall in the temperature of the steel in degrees centigrade.[1]

TABLE VI.—PYROMETRIC MEASUREMENTS OF RECALESCENCE.

Steel No.	Carbon.	Heat evolved at Ar 1 on cooling.		Heat absorbed at Ar 1 on heating.	
		Rise in Pyrometric Millimetres.	Equivalent rise in temperature of steel.	Fall in Pyrometric Millimetres.	Equivalent fall in temperature of steel.
	Per cent.		Degrees Centigrade.		Degrees Centigrade.
1	0·08	0·7	2·2	..	..
1½	0·21	2·7	8·4	..	..
2	0·38	9·1	28·2	..	..
3	0·59	15·0	46·5	10·5	32·6
3½	0·74	20·0	62·0	14·0	43·4
4	0·89	23·6	73·2	17·8	55·2
5	1·20	21·3	66·0	16·4	50·8
6	1·47	17·3	53·6	13·0	40·3

In the case of steels Nos. 1, 1½, 2 and 3, Ar1 being separate on cooling was calculated direct from the curve. In steels Nos. 3½, 4, 5 and 6 the total heat (being evolved in a single point) was calculated, and from it was deducted the heat evolved at Ar2–3 in steel No. 1. In the heating experiments on steels Nos. 3, 3½, 4, 5 and 6, the total heat absorbed was calculated, and from it was deducted the heat absorbed in No. 1 steel at Ar2–3. It will be seen that in both curves there is a sharp return break at 0·89 per cent. of carbon, as demanded by the Author's theory; in fact, the return curve is distinctly steeper than that required theoretically. (The heat from No. 6 steel should be only about 10 per cent. less than that evolved from No. 4, see Table IX.) This discrepancy is probably due to a slight dissociation of carbon and iron at the change point absorbing a little heat. Analyses on four grammes of turnings from the recalescence pieces of Nos. 5 and 6 steels gave the graphite by combustion respectively as 0·05 per cent. and 0·29 per cent. This separation of graphite is evidently brought about by the somewhat slow cooling after the first prolonged heating at 1,000° C. to remove the gases from the steel.

Figs. 26 illustrate graphically the fact that, on heating a highly graphitic steel (No. 6 annealed) the graphite to a great extent

[1] The minute error due to the increase in the specific heat of the mass as the carbon rises, lies well within the limits of error of the experiment.

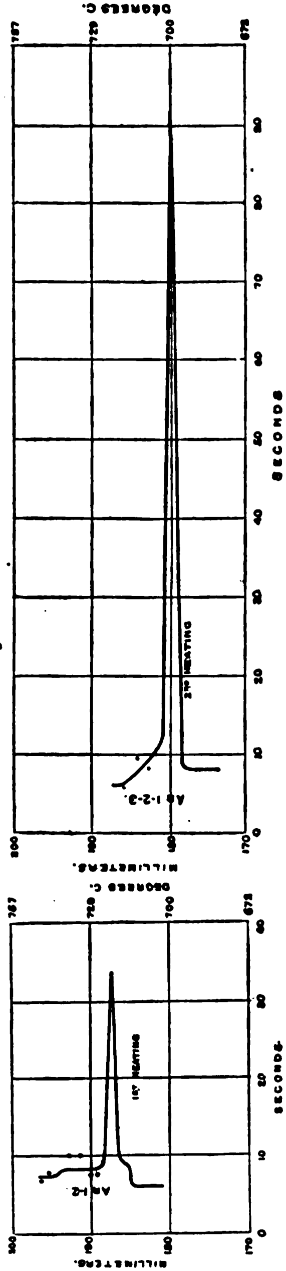
recombines with the iron when the metal is raised to a full red heat. This is indicated by the much greater absorption of heat at the carbon change point on the second heating.[1]

The thermal results correlated with the micro-structure strongly confirm the Author's view that the heat evolved or absorbed at Ar 1 is due not to a change of dissolved free carbon to Fe_3C, or of Fe_3C to dissolved carbon and free iron, but to a carbonization of the sub-carbide $Fe_{24}C$ to Fe_3C, or a reduction of Fe_3C to $Fe_{24}C$. Thus, respectively,

1. $Fe_{24}C = Fe_3C + 21\ Fe$ (Heat evolved).
2. $Fe_3C + 21\ Fe = Fe_{24}C$ (Heat absorbed).

It will be noted that in the lower part of the cooling curve there is a well-marked loop. This is not due to errors of observation; indeed, a prolonged exposure of such steels to a full white heat *in vacuo* largely increases the magnitude of the loop. This is due to a fact to which the Author directed attention in a Paper on "The Physical Influence of Elements on Iron,"[2] viz., that under the above thermal conditions the heat normally evolved at Ar 1 can be almost totally transferred to the critical point Ar 3. This phenomenon is connected with the pale brown iron crystals described in the

Figs. 26.



Recalescence Curves of No. 6 Steel after Annealing.

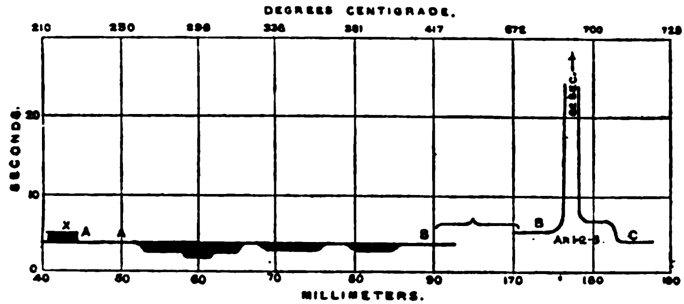
[1] A micro-section of the recalescence piece was found to be devoid of iron crystals.

[2] Journal of the Iron and Steel Institute, No. 1, 1894, p. 107.

microscopical section as probably containing a small quantity of an intermediate carbide $Fe_{10}C$.

The Tempering of Steel.—It has been seen already that sub-carbide of iron is chemically a very unstable compound; its thermal instability is also very remarkable; *Fig. 27* shows graphically the decomposition of 115 grammes of sub-carbide, that is to say, of the recalescence test-piece of No. 4 steel suddenly quenched out from a full red heat. It is, however, very difficult to show graphically the whole of the heat evolved from the carbonization of the sub-carbide to normal carbide; because the reaction commences at a temperature as low as 100° C., being merely marked by an even acceleration of the rate recorded on heating a hardened steel compared with the rate observed *cæteris paribus* on heating a normal steel. Such evolution would have to be represented by a

Fig. 27.



HEATING-CURVE OF NO. 4 STEEL (AFTER BEING HARDENED), SHOWING TEMPERING.

continuous level band as at X, *Fig. 27.* The change, however, acquires an increased velocity at about 250° C., reaches a maximum at about 300°, and is complete at about 400°. These evolutions of heat are shown graphically by the black areas. At AA the material is still principally sub-carbide; at BB it is a mixture of 87 per cent. of Fe and 13 per cent. of Fe_3C; whilst at C the latter has again, under the influence of a higher temperature, become reduced to $Fe_{24}C$. If the steel be now quenched, the cycle of changes can be repeated. To obtain a practical illustration of the mechanical effects of these changes, quench a small bar of iron containing 0·9 per cent. of carbon from a full red heat. The edge of the cold bar will readily scratch glass and strip the hardest file. Next, heat the bar for, say, fifteen minutes in a sand-bath at 300° C.; when cool, the metal will be found to be filed with ease. It will

be obvious that by arresting the change at various points by the agency of sudden cooling, the brittle properties of the adamantine $Fe_{24}C$ may be modified to any desired extent by converting a portion of it into (Fe_3C + 21 Fe), and so toughening, or as it were diluting, the remaining mass of $Fe_{24}C$ with particles of soft, free iron.

MAGNETIC PROPERTIES OF HARDENED IRON AND CARBON STEELS.

The supporters of the β iron theory regard the magnetic properties of Hadfield's manganese steel as affording strong evidence of the accuracy of their views, holding that its impermeability to magnetism is due to the alleged fact that in it the iron exists as the hard β modification, the molecules of which are non-magnetic. Mr. Osmond's contention, that because carbon steel at a full red heat is non-magnetic, and cold manganese steel is also non-magnetic, therefore both owe their impermeability to the presence of β iron, has always seemed to the Author an argument devoid of cogency. It is indeed generally admitted that the molecular condition of suddenly quenched steel approximates to that of the fully red-hot metal; and hence it is obvious that if both quenched carbon steel and manganese steel consist chiefly of β iron, which is non-magnetic, therefore quenched carbon steel is non-magnetic, which is absurd. In a Paper read before the Iron and Steel Institute in 1894,[1] Mr. Hadfield expressed an opinion that careful research would reveal a close connection between the magnetic properties of steel and the carbides of iron contained therein. The results of the experiments about to be described fully confirm the correctness of that surmise.

The bars employed (including a bar of Hadfield's quenched manganese steel) were 120 millimetres long by 10 millimetres in diameter. They were slowly heated in a vacuum to a temperature of 850° C., and were then suddenly quenched in a tank of cold water. After being gently polished in the lathe with fine emery cloth, they were magnetised for five minutes in a wooden reel round which was coiled over 1,000 turns of wire. The current was from five storage cells, and its constancy was proved by an ammeter placed in the circuit. The comparative permeability of each bar was determined by means of a magnetometer, consisting of a short magnetised bar suspended in a large bell jar by a single fibre of silk. The small magnetised bar was fitted with

[1] Journal of the Iron and Steel Institute, No. 1, 1894, p. 156.

long glass pointers, so that the degrees of deflection could be read off to 0·1°. The comparative permanent magnetism of the bars was determined after an interval of twenty-four hours, the magnets also having been each dropped fifty times on to wood from a height of 2 feet 6 inches, so as to eliminate the sub-permanent magnetism. Special precautions were taken to avoid any mutual induction effects by the bars. The comparative magnetic intensities were then determined by deflection and by vibration. The latter tests were made in a bell-jar in which the magnets were balanced in a wooden sling suspended by a few fibres of silk. The time of ten vibrations was taken through a telescope provided with a cross-wire by a stop-watch graduated to one-fifth of a second. The results of the deflection and vibration tests closely agree, excepting in the case of steel No. 1½, a discrepancy for which the Author cannot account.

The readings obtained are set forth in Table VII, and are plotted as curves in *Figs. 28* and *29*.

TABLE VII.—MAGNETIC OBSERVATIONS ON HARDENED STEELS.[1]

Steel No.	Carbon.	Magnetic Permeability.		Permanent Magnetism.			
		Magnetometer Deflections (Coil and Bar).[2]	Tangents.	Magnetometer Deflections.	Tangents.	Times of one Vibration.	Inverse Squares.
	Per cent.	°		°		"	
1	0·08	36·25	0·73323	3·75	0·06554	59·60	2·81
1½	0·21	35·10	0·70281	12·00	0·21255	40·60	6·07
2	0·38	34·30	0·68215	19·50	0·35412	26·56	14·17
3	0·59	32·00	0·62487	31·00	0·60086	20·36	24·12
3½	0·74	31·75	0·61882	32·00	0·62487	20·10	24·75
4	0·89	29·00	0·55431	38·50	0·79543	17·98	30·93
5	1·20	25·50	0·47698	38·85	0·80546	17·80	31·56
6	1·47	24·40	0·45362	38·50	0·79543	17·88	31·28
Hadfield's manganese steel	1·00	13·60	0·24193	0·00	..	Would not vibrate	..

From these results (and from others not detailed herein) the Author feels justified in provisionally stating the following laws *cæteris paribus* for quenched iron and carbon steels.

[1] The deflections in the permeability and permanent magnetism columns are of course not comparable. The bars in the measurements last named were much closer to the instrument than the magnetizing coil. The deflections from the latter for steels Nos. 4, 5 and 6 were all 9°·5 for permanent magnetism.

[2] Deflection from coil *per se* 13°·5.

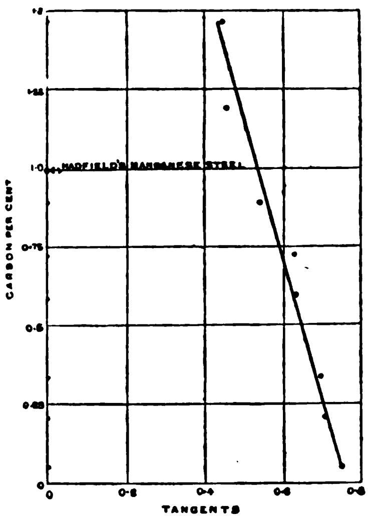
1. The magnetic permeability varies inversely as the carbon present.

2. The permanent magnetism is directly proportional to the carbides of iron present.

3. In iron containing between 0·1 per cent. and 0·9 per cent. of carbon, the permanent magnetism is directly proportional to the sub-carbide of iron present.

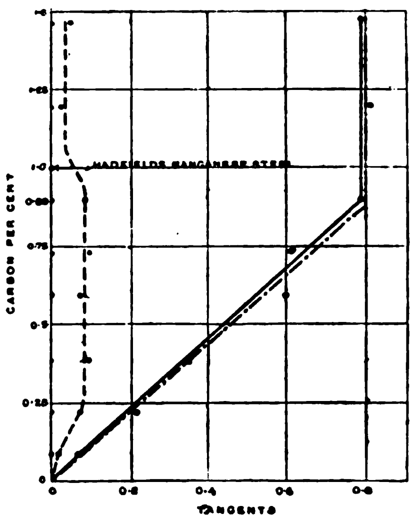
It will be observed that the curve of permanent magnetism presents a sharp break at 0·89 per cent. of carbon, identical with that observed in the thermal and in several of the mechanical

Fig. 28.



Line showing comparative magnetic permeability of hardened steels.

Fig. 29.



Observed curve showing comparative permanent magnetism of hardened steels . . —·—·—
Theoretical ditto . . . . ———
Observed ditto, after tempering for thirty minutes at a temperature of 300° . . – – – –

curves, and exactly corresponding with the microscopical saturation point.

If the third statement be true, it follows that a change involving the conversion of the sub-carbide into normal carbide should be followed by a corresponding falling off in the permanent magnetism of hardened carbon steels. On referring to the tempering-curve of No. 4 steel, *Fig. 27*, it will be seen that the carbonization of the lower to the higher carbide acquired a maximum velocity at 300° C. Accordingly, the set of magnets were heated in a closed sand-bath at that temperature for half an hour. The

results obtained are set forth in Table VIII, and the curve of the residual magnetism is plotted with that of the quenched steels in *Fig. 29*. An inspection of the Tables and curves will render almost unnecessary the remark that between quenched carbon and manganese steels there is not the slightest magnetic analogy.

TABLE VIII.—SHOWING INFLUENCE OF TEMPERING ON THE PERMANENT MAGNETISM OF HARDENED STEELS.

Steel No.	Carbon.	Permanent Magnetism before tempering.		Permanent Magnetism after tempering thirty minutes at 300° C.	
		Magnetometer Deflections.	Tangents.	Magnetometer Deflections.	Tangents.
	Per cent.	Degrees.		Degrees.	
1	0·08	3·75	0·06554	0·3	0·00524
1½	0·21	12·00	0·21255	4·3	0·07519
2	0·38	19·50	0·35412	5·3	0·09277
3	0·59	31·00	0·60086	4·2	0·07344
3½	0·74	32·00	0·62487	5·3	0·09277
4	0·89	38·50	0·79543	4·6	0·08046
5	1·20	38·85	0·80546	1·6	0·02619
6	1·47	38·50	0·79543	2·6	0·04541

It is perhaps premature to speculate on the comparative magnetic properties of the normal carbide and the sub-carbide of iron; but the set-back in the upper portion of the curve of the tempered magnets seems to indicate a possibility that the equal magnetic intensity of the hardened steels above the saturation point is maintained by the inductive action of the sub-carbide on the surplus normal carbide of supersaturated steels. It has been stated that the change of the sub-carbide to the normal compound commenced at about 100° C. It was also found that on heating hardened magnetised bars for thirty minutes in an air-bath at 105° C., a falling off of several degrees was noted in the magnetometer deflections.

Much light will doubtless be thrown upon the magnetic characteristics of the two carbides of iron by a series of experiments on the comparative magnetic properties of normal annealed and hardened steels; and for such an investigation the Author is now preparing, having recently succeeded in making a set of rolled bars similar to those described in the present Paper, but in which the total impurities amount to only about 0·1 per cent. As the result of preliminary experiments, it may be remarked that there is no doubt that the magnetism of unhardened steels is chiefly sub-permanent.

GENERAL SUMMARY.

1. The constituents of steel may be:—(*a*) Crystals of pure iron which remain bright on etching. (*b*) Crystals of slightly impure iron which become pale brown on etching, probably owing to the presence of a small quantity of an intermediate carbide of hypothetical formula $Fe_{10}C$. (*c*) Normal carbide of iron, Fe_3C, which exists in three distinct modifications, each one conferring upon the iron in which it is found particular mechanical properties. (1) Emulsified carbide present in an excessively fine state of division in tempered steels. (2) Diffused carbide of iron occurring in normal steels in the forms of small ill-defined striæ and granules. (3) Crystallised carbide of iron occurring as well-defined laminæ in annealed and in some normal steels. (*d*) Sub-carbide of iron, a compound of great hardness existing in hardened and tempered steels and possessing the formula $Fe_{24}C$. This substance is decomposed by the most dilute acids, and at 400° C. it is decomposed into Fe_3C and free iron with evolution of heat. One of the most remarkable properties of this compound is its capacity for permanent magnetism. (*e*) Graphite or "temper-carbon."

The existence of $Fe_{24}C$ is proved by the fact that iron containing 0·89 per cent. carbon presents several correlative critical points when examined by different methods of observation:—(1) Well-marked saturation-points in the micro-structure of normal annealed and hardened steels. (2) A sharp maximum in a curve the co-ordinates of which are heat evolved or absorbed at Ar 1 and carbon percentage. (3) A point in the compression curve of hardened steels at which molecular flow absolutely ceases. (4) A sharp maximum in a curve the co-ordinates of which are carbon percentage and permanent magnetism in hardened steels.

II. The influence of annealing is—(1) To increase the size of crystals and to increase the inter-crystalline cohesion when originally feeble or impaired. (2) To convert elongated masses of iron containing diffused Fe_3C into compact rounder bodies, containing laminæ of crystallised Fe_3C, between which the iron becomes more or less dovetailed throughout the mass.

III. The approximate theoretical constituents of hardened and normal steels will be in accordance with the figures given in Table IX. (These percentages, however, can never be quite correct, because in practice hardened steels below the saturation point always contain a little Fe_3C, and normal steels below the saturation point a small quantity of the intermediate carbide

$Fe_{10}C$ (?)). It is obvious that in tempered steels an almost unlimited variety of constitutions and consequently of mechanical properties is possible.

TABLE IX.—APPROXIMATE THEORETICAL COMPOSITION OF HARDENED AND NORMAL IRON AND CARBON STEELS REQUIRED BY THE SUB-CARBIDE THEORY HEREIN ENUNCIATED.

Carbon.	Hardened Steels.			Normal Steels.	
	Fe.	$Fe_{24}C$.	Fe_3C.	Fe.	Fe_3C.
Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
0·1	89	11	0	99	1
0·2	78	22	0	97	3
0·3	67	33	0	95	5
0·4	56	44	0	94	6
0·5	45	55	0	93	7
0·6	34	66	0	91	9
0·7	22	78	0	90	10
0·8	11	89	0	88	12
0·9	0	100	0	87	13
1·0	0	99	1	85	15
1·1	0	97	3	84	16
1·2	0	95	5	82	18
1·3	0	93	7	81	19
1·4	0	91	9	79	21
1·5	0	89	11	77	23

IV. The sub-carbide theory falls into line with the observations of every-day experience. For instance, the fact has long been known that pure carbon steel, containing about 0·85 per cent. of carbon, is the most suitable for steel which must carry a cutting-edge and yet be tough enough to withstand a sudden shock. Such steel is therefore employed for cold setts. It is also well known that a steel containing 1·3 per cent. of carbon would be useless for such a purpose, as it would crack and "snip." The reason is clear; such material is full of lines of weakness along the junctions of the sub-carbide granules with the surplus normal carbide membranes. On the other hand, it is known that a steel harder than one carrying 0·9 per cent of carbon is necessary for turning-tools. In such a case no shock has to be encountered, so that the surplus Fe_3C augments the hardness of the sub-carbide with its own intense hardness, and moreover adds 10 per cent. of a substance incapable of "letting down" with the heat of friction. It is also clear that a steel with carbon much below 0·9 per cent. cannot carry a cutting-edge, because of the presence of particles of soft free iron amongst the mass of the hard sub-carbide.

(It is important from a practical point of view, to bear in mind

the fact that on heating a saturated steel in air, the surface of the metal will fall below the saturation point owing to the atmospheric oxidation of some of the carbon, and that such loss is irremediable; whereas in a steel containing, say, 1·2 per cent. of carbon, the surplus membranes of Fe_3C act as reserves to replace carbon burnt off, and so maintain the metal in a saturated condition.)

The principles herein enunciated are directly applicable to the higher grades of pure iron and carbon steels, but it will be obvious that in other directions they form only a base of operations. In connection with the interminable contest between armour plates and armour-piercing shells, a scientific basis of manufacture will not be arrived at until a closer study has been made of the properties of the double carbides formed with iron by silicon, manganese, nickel, chromium and tungsten.

In conclusion, the Author has to tender his best thanks to Professor Ripper, Principal of the Sheffield Technical School, and to Mr. G. C. Gulliver, for the kindness with which they have co-operated in preparing the large number of test-pieces required during the several years occupied by the research referred to; also to Mr. J. J. Jefferson, A.R.S.M., and Mr. F. K. Knowles, Demonstrators of Metallurgy, for their most valuable services in connection with the work; and to Mr. Ellis Crapper, Lecturer on Physics at the school, for his cordial assistance.

The Paper is accompanied by ten drawings and a set of nineteen photographs, from which Plate 4 and the *Figs.* in the text have been prepared.

(*Paper No. 2931.*)

"The Dilatation, Annealing and Welding of Iron and Steel."

By Thomas Wrightson, M. Inst. C.E.

In studying the properties of iron and steel, Civil Engineers have been accustomed to deal with these metals in their final condition only, and to leave to chemists and physicists those scientific considerations of manufacture which so largely affect the ultimate characteristics of these metals. The Author does not propose here to treat of chemical considerations; these have been closely investigated by others. His observations will be confined to some of those less understood physical changes which take place in iron and steel, and which are probably quite as important as any chemical changes in influencing the final properties of these materials. Some of these allotropic changes have recently been the object of study, and the labours of Roberts-Austen, Osmond and others, have done much to bring them into prominence.

Every ounce of iron and of its carbon alloy, steel, used by Members of the Institution has at one time been in a liquid and homogeneous condition. When it reaches the engineer's hands, although chemically it may be the same, its condition as to temperature, molecular structure, and other physical properties is entirely altered. The change and development of these physical properties merit careful study by those to whom its final qualities are of such vast importance; and in such an examination there appears to be the most hopeful prospect of understanding and neutralizing the causes of the treacheries in these materials which give so much anxiety, and necessitate the use of those high factors of safety in structural design which form the chief barrier to economy in engineering practice.

There appears, therefore, to be ample justification for investigating minutely the physical changes which occur as the material passes from the homogeneous condition known as the molten state to the solid and more permanent condition in which it is expected to realise all the hopes, expectations and assumptions of the

engineer. On these grounds the Author makes no apology for bringing before the Institution some results of physical investigations dealing with this subject which have occupied his attention for the last fifteen years.

DILATATION.

Fallacious and unverified assumptions made by men regarded as authorities have frequently, in the history of science, been the means of retarding progress. The solution of problems connected with the treacheries of iron and steel has been especially retarded by the unfortunate assumption that the dilatation of these metals was continuous and uniform from the solid to the liquid state. The Author can remember a long correspondence in the engineering journals on the curious anomaly that although, in making an iron casting, the pattern for moulding it had to be made about 1 per cent. in all its lineal dimensions larger than the dimensions desired in the casting—implying a contraction of the liquid metal in cooling—yet if the same casting was, when cool, put into molten iron it floated. A recognised metallurgical authority of that time, Mr. Robert Mallet, F.R.S., experimented upon the question, and read a Paper[1] before the Royal Society, in which he claimed to show that the floating of solid cast-iron on liquid cast-iron was not due to change of specific gravity, as the iron undoubtedly contracted in cooling, and should therefore sink. In his own words, "The Author finds himself justified in concluding that it is not true that any cast-iron is denser in the fused than in the solid state." Cold cast-iron he therefore contends does not float upon liquid cast-iron of the same quality by reason of its buoyancy, but in virtue of some other force. He adds, "What is the nature of the force which produces this curious phenomenon, and often in direct opposition to gravity, is a different and a much more delicate and difficult inquiry, which he must leave to physicists to fully investigate."

Without going into the details of Mr. Mallet's experiments, it may be stated that his arguments rested upon two assumptions, neither of which has been subsequently proved to be correct.

He assumes (1) that cold cast-iron really does float on liquid iron; (2) that dilatation is continuous and uniform in cast-iron from its solid to its liquid state.

[1] "On the alleged expansion in volume of various substances in passing by refrigeration from the state of liquid fusion to that of solidification." Proceedings of the Royal Society, vol. xxii., 1873–74, p. 366.

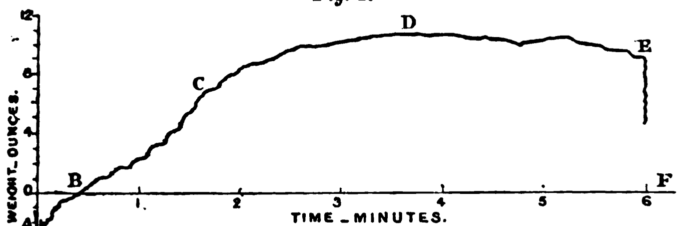
In the first place, solid cold cast-iron does not float in liquid cast-iron. It does so apparently, but only if the experiment be carried out carelessly (which it generally is), viz., by throwing a piece of cold solid cast-iron by hand into a ladle of molten iron. It certainly first sinks and then comes up, appearing to resemble the action of a piece of wood, which when thrown into water first sinks and then rises; but the cases are not comparable, as there is a very great difference between the heat of the molten iron and the cold iron, for which there is no parallel in the case of the wood and water experiment. What happens is this:—The iron sinks below the surface for two reasons—firstly, because it is denser than the liquid, and secondly, because it is thrown in, attaining a velocity which carries it down. The heat from the liquid iron is then conducted into the solid ball, causing gradual expansion, and consequent displacement of the liquid iron, which, according to the law of floating bodies, produces increased buoyancy which brings the ball to the surface. As the result of numerous experiments the Author has found that if, instead of throwing the piece of solid iron into the liquid iron, it was lowered by means of an iron fork, it always followed the fork down, but in a few seconds left the prongs and floated to the surface.

The second assumption, that the dilatation was continuous and uniform, is also erroneous. This becomes apparent from the behaviour of a sphere of cast-iron after first sinking and then appearing at the surface. For some time the sphere continues to rise, showing a considerable proportion of its whole cubical capacity above the liquid surface, until it reaches the temperature at which it melts, when it quickly joins the bath of molten iron. The experiment shows the ball first sinking, proving itself denser than the liquid iron, then expanding and becoming considerably less dense than the liquid, and lastly, a reversal taking place, and the metal in melting contracting in volume and becoming of the same density as the liquid it joins. This observation of the very large proportion of the volume of the cast-iron ball becoming emergent before melting led the Author in 1879–80 to undertake experiments upon this interesting phenomenon. In the crude experiment the top of the ball being emergent caused two disturbances which should in a more accurate investigation be eliminated. First, the floating scoria hindered the free rising and accurate observation of the ball. Second, the lower part of the ball being immersed in fluid metal and the upper part rising gradually into the air, there was a great difference in temperature between these different parts of the ball. Both these

disturbing influences could be got rid of if the ball could be kept well below the surface of the fluid metal.

In order to measure those changes of volume, the Author designed an instrument which would enable him to submerge entirely in molten iron a 4-inch solid ball of the same material. This was hung upon a spiral spring such as is used in a Salter balance, the connection between the spring and the ball being a rigid and sufficiently heavy rod to enable the ball to be kept from rising to the surface. As it heated and expanded, the tension on the spring became less by the increasing buoyancy of the ball, and as this increased buoyancy was, according to the law of floating bodies, exactly equal to the increased displacement of the liquid, or, in other words, equal to the increased volume of the ball, the contraction of the spring afforded a very simple and direct means

Fig. 1.



Weight of ball and immersed part of stalk	. . . .	132 ozs.
Maximum sinking effect	. . . .	2 ,,
,, floating ,,	. . . .	11 ,,
Specific gravity of ditto	. . . .	6·95
,, ,, fluid iron	. . . .	$\frac{6 \cdot 95 \times 130 \cdot 0}{132} = 6 \cdot 84$
,, ,, plastic metal	. . .	$\frac{6 \cdot 95 \times 130}{143} = 6 \cdot 32$

4-Inch Ball of No. 4 Foundry Iron (Cleveland).

of indicating the changing volume of the ball of metal as it gradually absorbed heat until it melted and rejoined the bath.

In order to register by diagram these changes, the spring was mounted in a frame, and to its moving end was attached a pencil, the point of which described a vertical line representing ounces of tension on the spring.

This pencil point pressed on a sheet of paper wound round a vertical cylinder which revolved by clockwork. By this means a diagram was produced of which *Fig. 1* is an example, in which F B represents the liquid volume, and A B C D E the changing volume in passing from the solid to the liquid state.

The vertical distance of the point A below the line F B represents the weight the solid cold ball exceeds the weight of the liquid metal displaced when first immersed. As the ball becomes

heated the pencil moves to B, at which time the weight of the displaced liquid coincides with the weight of the ball. It is at this point that a freely floating ball would have just appeared at the surface. The expansion beyond this corresponds to the gradual rising of a free ball above the surface which the original observation showed. As the ball becomes hotter, the curve flattens between C and D, the conduction of the heat into the ball becoming slower until no further expansion takes place.

The Author caused several balls to be removed at this stage, and found them complete in form owing to the high conducting power of the iron, but so soft that a steel pin could be pushed through them. This plastic condition remains for a short time when the pencil point falls quickly, showing that the ball has melted away and joined the liquid metal. Of course, so soon as melting begins, the pencil no longer registers measurable changes of volume, as a reduction of mass is taking place; but the maximum volume can be measured in the plastic condition, and the volume, when it reaches the liquid state, being known, it can be stated with certainty that it passes rapidly from one condition to the other.

The average of a number of experiments upon grey Cleveland iron led the Author to conclude that—

The specific gravity of the cold solid iron was	6·95
" " molten iron was	6·88
" " plastic iron was	6·50

In other words, cast-iron passing from the solid to the liquid condition has a minimum volume when solid.

As the temperature rises it first expands in volume 1·02 per cent. and at this point, although solid, is of the same density as the liquid metal. It then continues expanding until it reaches the plastic condition, when it assumes its maximum volume with a specific gravity of 6·5, the total increase of volume from the cold solid to the plastic state amounting to 6·92 per cent. After this, expansion by increased heat ceases, and a quick contraction occurs until the mass becomes liquid, when its specific gravity is 6·88.

If expressed in terms of the volume of liquid iron taken at	100·00
That of plastic iron is	105·85
That of solid iron at atmospheric temperature	98·98

These changes of volume were so remarkable and the results so different from those of Mr. Mallet, that the Author thought it advisable to reverse the order of experiment and to measure the change of volume as the molten iron solidified. Two spherical moulds of dried loam were made 15 inches diameter, and into these

molten iron was poured, in one case Cleveland white iron and the other Cleveland grey iron.

A few minutes after the iron was run, the top half of the mould was raised, and the diameter of the congealed surface measured carefully with callipers. This was repeated at intervals of time. The diagram, *Fig. 2*, shows the gradual increase of diameter of the grey and white balls as they cooled. The large size of the balls made the cooling a slow process and the horizontal line

Fig. 2.

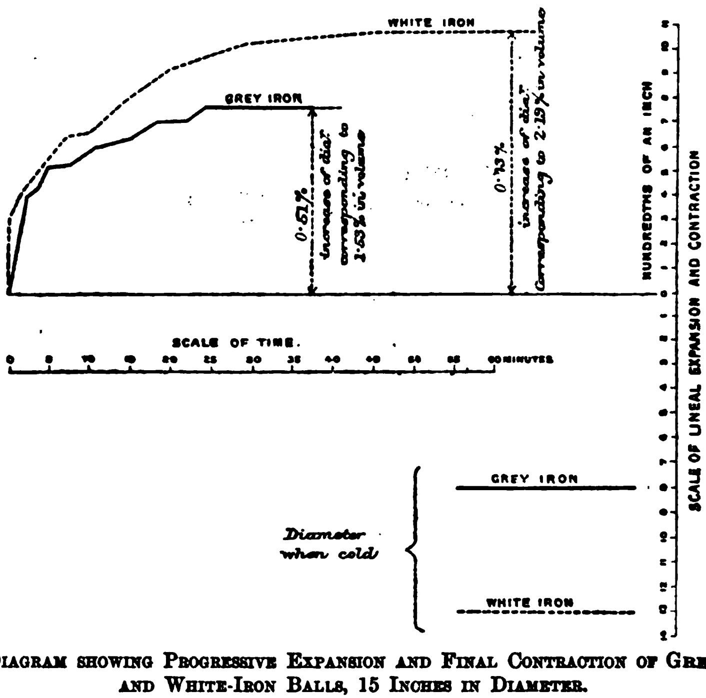


Diagram showing Progressive Expansion and Final Contraction of Grey- and White-Iron Balls, 15 Inches in Diameter.

representing time could not conveniently be extended in the diagram; but the final diameter when the balls, after ten or twelve hours, became cold, is shown, and the general result is a qualitative confirmation of the other experiments, although not quantitative, as the early consolidation of the outer layers prevents the free expansion of the interior.

Fig. 3 represents the appearance in section of the grey-iron ball when broken through the centre. The top cavity may be accounted for thus:—The metal first swells in the mould and as

the outside layers cool first and consolidate before the internal part has commenced to swell, the interior becomes short of liquid metal, which gravitates to the bottom and leaves the upper part vacant. In the ordinary course of casting, a man feeds the 'git' with fresh metal, keeping it from congealing by stirring the opening with a rod. The usual explanation for the cavity is that the metal contracts; but unless the outer shell expanded in the first place, the porosity and vacancy could not be so great, nor would it necessarily occupy the top of the mould, as it invariably does.[1]

Fig. 3.

Scale, ¼ full size.

CROSS-SECTION OF GREY-IRON BALL.

The whole process of change in the ball is, according to the Author's view, as follows:—

1. The fluid metal entirely fills the mould.
2. An expansion of the outer layers of the metal takes place in becoming plastic, thus increasing the diameter of the ball.

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[1] To further prove this two 9-inch balls were cast. No. 1 was turned upside down a minute after being run; No. 2 was also upturned, about five minutes after casting. In the former the cavity, instead of being at the "git," moved round to the opposite side, and in the latter the cavity was distributed in the form of a porosity about the central portion of the ball.

3. The liquid interior not having commenced to expand sinks down in the hollow shell formed by the cooling and expanding layers of the outside, and thus forms the cavity at the top.

4. The metal round the interior surface of the top cavity hardens.

5. The interior liquid metal expands gradually towards the centre, and by its pressure on the soft outer envelope continues to increase the diameter of the ball.

6. This goes on until the outer layers arrive at such a temperature that they should contract.

7. A contest then arises between the contracting force of the fast-thickening outer layers and the expanding force of the interior as it in turn becomes plastic.

8. When these forces balance each other further expansion is arrested.

9. After this point in the cooling has been reached, the outer layers contract as far as their condition will allow, but not to the full natural extent, as, while the outside was in a state of tension owing to the swelling of the interior, fresh layers of plastic and solidifying metal had been built up in the interior, which, by the time contraction had commenced, had formed an arch of many courses under different degrees of tension, and such a structure tended to prevent the free contraction of the whole mass.[1]

10. The interior of this enlarged vessel then contracts, and draws away principally from the upper part, owing to the mass of plastic iron tending to gravitate to the bottom of the ball.

Samples of iron were cut from the section of this ball at the positions indicated on the drawing, and their specific gravities taken, the results being as follows:—

	Specific Gravity.
At the surface of the top cavity, 5½ inches above centre of sphere	6·95
„ „ centre of sphere	7·13
„ a point 2⅜ inches below the centre	6·87
„ „ 4¾ „ „ „	7·08
„ „ 7⅛ „ „ „	7·15

As might be expected, the lowest point is that of greatest density, owing to the pressure consolidating the metal. The metal between the centre and the top cavity is very porous, and shows the tearing action of the force of contraction very plainly, resembling in appearance the well-known contraction

[1] It will be seen that the total contraction from the diameter of the mould to the final diameter of the ball was only 0·06 inch, whereas where free contraction is attainable it would have been about 1 per cent. of the diameter, or 0·125 inch.

cavities in steel ingots, and leading to the surmise that these may be produced by similar expansion and subsequent contraction. The ball in cooling only returned to the original diameter of the mould (15·28 inches) about five hours after it had been run, and when the outside of the ball was quite black. Observation of other castings has shown that the external surface is almost if not quite black before the iron has contracted to the original size of the mould.

Recent experiments have been made in co-operation with Mr. Bagley, of the Moor Steel-Works at Stockton, on the buoyancy of solid rolled low-carbon steel in fluid steel of the same quality. These experiments proved conclusively that this quality of steel follows the same law as cast-iron in sinking first for a few seconds, then gradually rising to a considerable extent above the surface of the bath. *Fig. 2*, illustrating experiments in measuring by callipers the white-iron ball, shows that the expansion of white iron in cooling within certain limits of temperature is even greater than the corresponding effect in grey iron. It appears, therefore, that in all these three carbon alloys of iron, viz., grey iron, white iron, and low-carbon steel, the physical changes from liquid to solid or from solid to liquid are similar, and are not by any means of that simple character which they would be were the theory of uniform dilatation correct. The result of the contest which has been described between different groups of particles is the creation of initial strains of varying kinds in different parts of the casting. Thus, in cooling, the condition of certain groups of particles may be one of expansion, while at the same moment the condition of neighbouring particles may be one of contraction, in which case internal strains must be generated; or a certain group of particles may be lowered in temperature just beyond the expanding condition, say to a temperature corresponding to the volume at the point D in *Fig. 1*, while a neighbouring group has a temperature corresponding to the volume at the point C; in this case it is evident that internal strains are set up which, if not in some way neutralized, will affect the permanent strength of the casting. When once these considerable changes in volume during cooling from liquid to solid are appreciated, no surprise can be felt at the treacheries which so frequently appear in structures of cast-iron and steel.

The Author has described the successive steps in the cooling of a 15-inch ball, but the same operations are at work in the cooling of every casting, modified in their effects to a greater or less extent by varied form or size. The engineer, in designing any casting should therefore avoid forms in which extreme initial

strains are likely to be generated. He should also see that the method of cooling is favourable to the avoidance of such strains being set up. It is not an uncommon thing for cast-iron plates of good material, after being cooled, suddenly and spontaneously to break into pieces, sometimes with a loud report. This arises from improper cooling. If too soon after the solidification of the plate it is removed from the sand and the cold air allowed to play upon it, some parts will cool more rapidly than others, and internal strains are set up which may come so perilously near the breaking-point, that although to all appearance the casting is perfectly sound, yet the slightest additional strain causes rupture.

Annealing.

The process of slow cooling, or annealing, anticipates this by causing the lowering of temperature and consequent change of volume to be so slow that the different groups of particles in the casting have time to accommodate themselves to their changing condition of volume and thus to minimize the internal strains which lead to treachery. The object of slow cooling is not solely to retard the fall of temperature of the whole mass. In every cooling body the radiation of heat from the exterior is more rapid than from the interior. The most important condition in slow cooling to be considered, in the light of the foregoing experiments and reasoning, appears to be that the difference of temperature at any moment during cooling between the hottest and the coldest particles is reduced to a minimum. Referring again to *Fig. 1*, if the highest and lowest temperatures correspond to D and C, then the cooler particles with volume C will, as they further cool, reduce in volume, while simultaneously the hotter particles with volume D will reduce to a less extent, the difference causing a permanent strain in the line joining these groups of particles.

By annealing, or slow cooling, the points D and C are brought vertically closer together throughout the whole process, thus minimizing the variation of volume between contemporary extremes of temperature and avoiding excessive normal strains being set up.

Welding.

This remarkable property of grey cast-iron, expanding nearly 6 per cent. in cooling from the liquid to the plastic condition, appeared to resemble the well-known property of water when

solidifying into ice, which expands 9·3 per cent. in volume in cooling from 4° C. to 0° C. One of the most interesting properties of ice is that of regelation, or the pressing of two wet surfaces of ice together, producing adhesion of the ice faces. The difficulty of explaining the phenomenon lies in the fact that the ice at the wet surface is at a higher temperature than the adjacent solid ice. When pressed it would be expected that heat should be generated and cause more melting of the solid ice. This is not the case, as it regelates and unites or freezes when pressure is applied. The anomaly was unexplained until Dr. James Thomson demonstrated from thermodynamic considerations that bodies which, instead of expanding by heat and contracting by cold, reversed the ordinary law, expanding in cooling and contracting in heating, must cool if subjected to pressure, and, further, that the effect of such pressure is to lower the melting-point of such a substance. This remarkable forecast, arrived at on purely theoretical grounds, was experimentally proved to be true by Lord Kelvin in a classical experiment, which was described before the Royal Society, Edinburgh, in January, 1850. In this experiment it was conclusively shown that the freezing-point was lowered 0·0075° C. for every additional atmosphere of pressure. This scientific prediction and experimental verification has been accepted by modern scientists as containing the explanation of regelation in ice, viz., that when the wet faces of the ice are pressed together the melting-point is lowered, so that more water is formed, but that as the pressure also produces a fall of temperature, the water formed re-freezes as the pressure is relieved.

The natural question arose in the Author's mind, after his experiments proving the same anomaly to exist in the cooling of cast-iron, and to an extent approaching the expansion in water, whether the phenomenon of welding in iron might not in that substance be due to the same causes that produced regelation in ice. Further than suggesting such a possibility, he had no means at that time of following up the investigation. In the first place, cast-iron does not weld, and, therefore, unless it could be demonstrated that wrought-iron and weldable steels have the same property of expanding when being cooled, within the limits of the welding temperature, the phenomena of regelation and welding could not be scientifically identified.

Although the Author had ascertained that white iron, which is nearer the condition of wrought-iron and steel than grey iron, expands in cooling even to a greater extent than grey iron, yet the viscid nature of melted wrought-iron and the much higher tem-

perature required to fuse it, made it impossible to experiment with it in the same way that had been successful in the case of grey iron. These difficulties obliged the Author to put the investigation to one side, but in 1894 he determined to approach the solution of the problem from a different point of view.

It is well known that the condition known as the welding state of iron or low-carbon steel, is one which exists only within a very limited range of temperature. If the smith takes his bars to be welded out of the fire at too low a temperature, welding cannot be effected. On the other hand, if the iron is too hot a failure is also certain. The range of temperature, during which impact or pressure causes the union known as welding, is therefore within narrow limits and the familiar operation is really a critical one.

To identify the welding phenomenon in iron with the regelation of ice, it must be experimentally proved that the surfaces of the iron at the moment of welding are in that peculiar condition of matter in which an increase of heat causes a contraction, and a diminution of heat an expansion of volume. But as, according to the reasoning of Dr. James Thomson, matter possessing this peculiar property must also have the property of being cooled by impact or pressure, the identification of welding and regelation will be equally satisfactory if this collateral property of the cooling of welding iron when under pressure can be demonstrated.

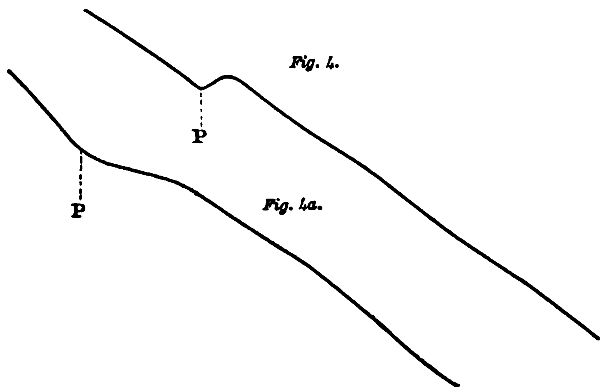
The recent successful application to metallurgical research by Prof. Roberts-Austen, of a recording pyrometer of great accuracy,[1] depending for its indications upon galvanometer measurements of electrical currents generated by a thermo-pile, appeared to encourage the hope that with such an instrument applied to welding faces the problem might be solved. On mentioning the suggestion to Prof. Roberts-Austen, he at once offered not only to place his laboratory and appliances at the Author's disposal, but to aid by his advice in the investigation. The first experiments were made by placing a thermo-junction between the two welding faces of bars heated in an ordinary smith's fire. The wires of the thermo-couple were carried from the smith's shop to the pyrometer in the laboratory, but it was soon found that the amount of signalling required from one building to another, as pressure was put on the bars, made the arrangement awkward and the results lacked uniformity.

After full consideration it was decided that the only satisfactory way to proceed would be to heat the bars in an electric welding-

[1] Minutes of Proceedings Inst. C.E., vol. cx. p. 156.

machine with alternating currents, and on application to the Electric Welding Company, they readily put one of their appliances with a suitable dynamo at the Author's disposal. The conductors from the dynamo were carried over the intervening buildings to the laboratory, where they were connected to the electric welder. The recording-pyrometer was placed in the same room and the wires of the thermo-junction were carried from the welder round the walls of the laboratory to the galvanometer placed inside the camera of the recording-pyrometer.

Although Professor Roberts-Austen's pyrometer has been described before the Institution, it may be well to recall that the deflection of the galvanometer mirror, produced by the current in the thermo-pile, causes a spot of light to move horizontally across a plate in a photographic slide, which slide



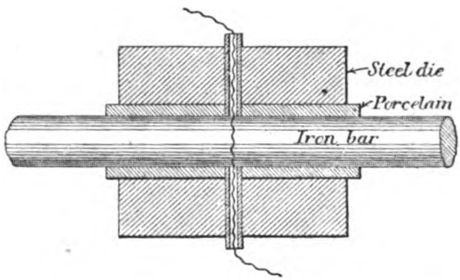
moves vertically at a uniform speed by clockwork. The spot thus describes a curve, the ordinates of which represent respectively temperature and time. A base line (also produced by photography) represents the zero of temperature, so that when the indications of the thermo-junction are calibrated, the diagram affords a complete record of the changes of temperature in the material in contact with the thermo-junction.

The first diagrams taken are shown on *Figs. 4* and *4a*. In these the current was cut off before the pressure was applied and the loss of heat through radiation is shown by the general fall of the curve from left to right.

It caused some passing disappointment to find that pressure applied at the time and temperature corresponding to the points marked P in the diagram, produced a rise instead of a fall in

temperature. On reflection it appeared that this rise might be due to the distortion and crushing up of the soft-heated iron, which, representing work done and heat evolved, might mask any true molecular fall of temperature. To eliminate this heat of distortion it appeared necessary to confine the bar inside a rigid case which, from the conditions of the experiment, must be a non-conductor of electricity. After many attempts and failures, the plan was adopted of placing the bar inside a close-fitting cylinder of porcelain outside of which was closely fitted a strong steel die; *Fig. 5* illustrates the arrangement of the thermo-junction. Both the porcelain cylinder and steel die are cut through the centre in a plane transverse to the axis. The wires of the thermo-pile may thus be led to the centre of the bar where the maximum heat is evolved. There was no reason for severing the bar at each experiment, as the object was not to make a weld but simply to observe the effect on the

Fig. 5.

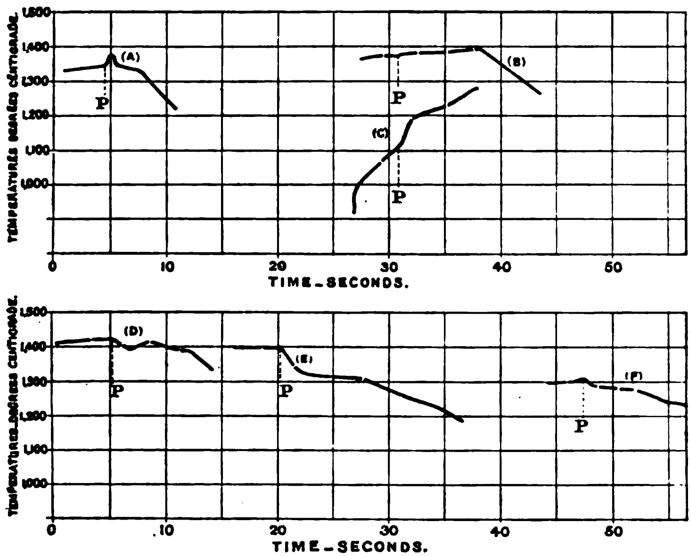


temperature when pressure was applied on iron which was known to be within the limits of the welding temperature. A continuous bar was therefore inserted with a minute hole drilled through the centre transverse to the axis, into which the thermo-junction was easily inserted. Pipe-clay insulators encircled the wires and rested in semi-circular grooves between the faces of the transverse cut through the die, so as to prevent contact between the die and the wire.

Arrangements were made to continue the welding current after the pressure was applied to such an extent as to counteract the radiation, which, in *Figs. 4* and *4a*, caused the rapid fall of the curve from left to right of the plate. It was also arranged that the time of this application of pressure should be identified in each diagram by momentarily obscuring the light which fell on the galvanometer mirror immediately before the pressure was put on, thereby producing a brief interruption in the continuity of the

curve. Four signals of this kind were arranged, so that the pressure should be applied between the first two and relieved between the last two. The welding current was arranged to be broken immediately after the fourth signal in each experiment, so that the different operations and their indications could not be mistaken in the subsequent examination of the diagram. *Fig. 6*, (A) to (F), shows a course of five experiments made under these conditions. The points P in each diagram indicating the moment when pressure was applied. In these the gradual rising of the curve before the application of pressure merely indicates that the amount of current

Fig. 6.



left in the welding machine was rather more than sufficient to counterbalance the loss of heat by radiation.

In the first experiment, (A), the pressure was applied when the temperature of the bar was 1,347° C. A sudden rise of 27° C. occurred. Such a rise might be expected until all interstices between the outside of the bar and the porcelain were filled by the plastic iron. The porcelain of course cracked, and the minute cracks had also to be filled before the effect of pressure producing a fall in temperature could be demonstrated.

In the second experiment the bar was, after an interval of time, heated to 1,371° C., and on pressing a rise of 8° only was shown

(B) and (C), the much reduced rise appearing to indicate that the interstices were nearly or quite filled.[1]

In the third experiment the same bar, under the same conditions, was heated to 1,420° C. On being pressed, a distinct fall of 27° C. was indicated on the diagram (D), thus for the first time realizing the anticipation with which the Author began the investigation.

In the fourth experiment, (E), the bar was heated to 1,400° C., when a pressure of about 1,200 lbs. per square inch on the section of the bar gave the remarkable fall of 57° C., lowering the temperature to 1,343° C.

In the fifth experiment, (F), the bar was heated to 1,300° C., a much lower temperature than the last, when, on being pressed, the fall in temperature was recorded as 19° C.[2]

The bar experimented upon (and placed on the table) shows a number of vein-like protuberances on its surface where the plastic iron had filled the crevices in the cracked porcelain before the final effect of pressure could cause a fall of temperature. From this series of experiments, it appears that the limits of temperature within which the thermal expansion of iron is negative certainly include a range between 1,300° C., and 1,420° C., and they may extend both below and above these temperatures, although the fall resulting from pressure is only 19° C. at the lower temperature as compared with 57° C. at the higher, it appears as if the lower temperature were approaching the lower limit. A series of experiments to determine the exact limits of this critical condition would no doubt be full of scientific interest.

With regard to a theory of welding which these observations lead to, if any one watches carefully two bars taken out of a smith's fire at a welding heat and pressed together, it will be seen that, immediately pressure is forcibly applied, there is perceptible to the eye a distinct increase in the mobility of the iron adjacent to the welding surface. The theory of regelation in ice, as held by the highest scientific authorities, is founded on the fact that the melting-point of ice is lowered by pressure. This, no doubt, holds equally good for iron, as proved by the property the Author hopes he has been able to demonstrate, but in the case of iron, it does more. Between the temperature of 1,400° C., and that of melting wrought-iron (stated to be about 1,600° C.) there are increasing degrees of mobility. When pressure is applied to a bar, say

[1] (C) is a diagram produced at the same time as (B), but from a more sensitive galvanometer. The same scale of temperature is not applicable.

[2] See also Philosophical Transactions of the Royal Society, vol. 186, pp. 593–602.

at 1,400°, it not only lowers the temperature of the melting-point, but increases the mobility at all lower temperatures within the critical condition, so that if, before pressure, the temperature is 1,400°, after pressure there may be a condition of mobility between the molecules which corresponds with a temperature without pressure of, say, 1,500°, although the temperature has been actually reduced by pressure to 1,343° C., as in experiment (E).

If two pieces of iron or almost any metal be raised to the melting-point, union can be effected; but this is by melting together, and not by welding. The process of welding appears to be that by which complete union can be effected by hammering or pressure at a temperature considerably below that required to melt the material. The heat of the fire having raised the bars within the critical range of temperature described, the smith in striking with the hammer is assisted by the special properties of the welding material in producing an increased mobility of the molecules which approach though do not arrive at liquidity. This condition is favourable to the interpenetration of the molecules and consequent adhesion of the surfaces.

The Author does not claim in this Paper to have exhausted the subjects with which he has dealt. He has only touched the boundaries of a large field of enquiry. What he desires to emphasize is that these physical changes are not of that minute kind which is generally the characteristic of molecular change. An increase of volume in iron, amounting to 6 per cent., in passing from one state to another, the whole of which change is in a direction opposed to our general experience of the effect of heat upon matter, cannot be regarded as insignificant, however much it may have been overlooked, and the Author trusts that he has been able to show in his remarks on the welding and annealing of iron and steel that a systematic study of these critical changes is necessary before the practical knowledge can be attained that will lead to the elimination of the treacheries in materials which are indispensable to the engineer.

The Author's thanks are due not only to Prof. Roberts-Austen, but to the Electric Welding Company, especially to Mr. Armstrong, the Manager, and Mr. Relf, the Electrician, who assisted in installing the machinery at the Mint and helped in every way to make the arrangement effective; also to Mr. R. A. Hill, Superintendent of the operative department of the Mint, who allowed the dynamo to be driven from the Mint engines.

The Paper is accompanied by drawings, from which the *Figs.* have been prepared.

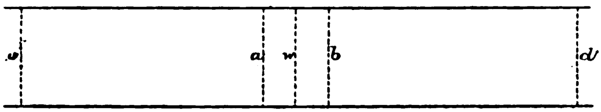
[APPENDIX.

APPENDIX.

Graphic Representation of the Author's Theory of Welding.

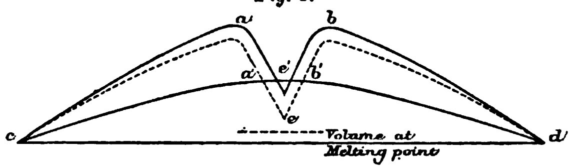
Fig. 7 represents diagrammatically an iron bar to be welded at *w*. Between *a* and *b* the material is in the critical condition in which pressure causes a fall

Fig. 7.



of temperature. Between *a* and *c* and between *b* and *d* the temperature of the iron is supposed to decrease gradually, and is in the ordinary state in which pressure increases temperature. When the welding surfaces are just in contact, and before pressure is applied, the volume curve through *c d* would be as shown by the upper full lines in *Fig. 8*. When pressure is applied the volume curve would be altered to that indicated by dotted lines, the volume of highest temperature, *e*, being brought nearer that of the melting-point (marked by the

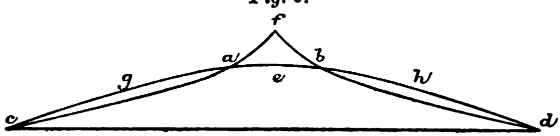
Fig. 8.



horizontal dotted line), which is somewhat greater than that of the cold solid. At this stage adhesion of the welding surfaces takes place by inter-penetration of the molecules. Further pressure or hammering would then compress the iron, bringing *a* and *b* down to *a'* and *b'*, and the parts between *a* and *c* and *b* and *d* down to *a' c* and *b' d*. During this period the radiation of heat is also diminishing the distance between *a* and *b* (the limits of the critical state), bringing them to *a'* and *b'*. The iron intercepted between these two points in cooling by radiation expands into the position *e'*, thus producing a uniform curve of volume, *c a' e' b' d*, which flattens into a straight line, *c d*, as the bar becomes cold.

The temperature through the same points is given in *Fig. 9*, rising gradually from *c* and *d* to *a* and *b*, and then rising with greater intensity to *f*. When

Fig. 9.



pressure and hammering are applied along *c d*, the part in the critical condition *a f b*, would cool to *e*, while the parts between *c* and *a* and between *d* and *b*, being in the ordinary condition of matter, would expand, thus producing a uniform curve of temperature, *c g e h d*, which flattens into a straight line as the bar becomes cold. The section *c d* of the bar through the weld thus becomes ...neous throughout as regards both density and temperature.

Discussion.

Sir Benjamin Baker.

Sir BENJAMIN BAKER, K.C.M.G., President, said the Institution had on many occasions been indebted to metallurgists and physicists, from whom also it looked for great assistance in the future. It was, therefore, always glad to welcome Papers of research such as those just read, although it might not be prepared to accept all the theories advanced. Some members might have thought that the Papers were somewhat remote from the daily work of engineers, but they would find on consideration that exactly the contrary was the fact—that the Papers were not merely intimately associated with the work of engineers, but were also of great interest even to the public at large. On many occasions accidents arose: a tire broke, or an axle, or a rail, and loss of life might result. Under such circumstances the directors of the company, in the present boasted state of advanced scientific knowledge, appealed to the engineer of the line, with the utmost confidence, to tell them the cause of the accident; and if there was one thing more humiliating to an engineer than to have to say, "I do not know," it was to add, "And I do not know anybody who can tell me." He remembered several cases in which broken rails had led to the loss of life. He particularly recollected three cases, on three different lines, one on the Continent and two in Great Britain. In one case a 21-foot rail had broken into twenty-two pieces. He believed Prof. Arnold considered that steel might be regarded as an igneous rock. Certainly the rail which he had seen might, from its appearance alone, have been regarded as an igneous rock, or as glass, for it had broken as if it had been a glass rail dropped on the pavement. On testing the steel the tensile strength had been found to be 42 tons to the square inch and the elongation 19 per cent. in 10 inches, which was equivalent to 25 per cent. in 2 inches. That was not much like igneous rock. The chemical analysis was everything that could be desired —carbon and manganese, each about 0·45 per cent., sulphur and phosphorus, about 0·08 per cent., and silicon, 0·12 per cent. Why did the rail break? He hoped metallurgists and physicists would continue their researches until engineers were able to tell railway directors why such things happened. He agreed with Prof. Arnold as to the importance of microscopic examination, because

Sir Benjamin Baker.

he was certain that in many cases old experimenters had omitted to look for surface cracks, or slight fissures. It was well known by Sir Frederick Bramwell and the Director-General of the Ordnance Factories, and by himself from experience, that in every gun, on the slope of the powder chamber, sooner or later very fine cracks extended in the steel radially from the outer surface inward. He did not know whether Wöhler, in his historic series of experiments on the repetition of stress, had examined the broken specimens for those surface cracks. He had himself made similar experiments, and had found that such cracks did occur. In a bar of steel, whatever might be its constituents, or a bar of iron of the toughest character, if it was bent backward and forward many millions of times, and then carefully examined, there would be found slight hair-like cracks extending from the surface inward. The same thing would be found in every gun after the firing of a certain number of rounds, whatever the quality of the steel. He had found it in the case of steel which had been heated to blue heat and broken in a testing machine when hot. The same thing had some years ago been experienced by Professor Unwin, with whom he had compared notes. A piece of steel had been tested at blue heat, and it had afterwards spontaneously fractured. That was the kind of thing upon which physicists were desired to throw light. In some of the rails which had been broken numerous hair-like cracks had been found in the head of the rail. Why did these occur? Was it the punishing or cold rolling of the steel? because even when there was no skidding action the same cracks had occurred. Similar fissures were also found occasionally in tires. At present he could not picture to himself what was the particular action which caused those very fine hair-like cracks when the steel was subjected to severe work, by powder pressure or some other cause. No doubt there were some physicists present who had thought deeply on the molecular movements which occurred in a bar even at rest; and it would be a great advantage to the Institution if they threw some light on those questions. The Papers, as he had said, were not simply of a scientific nature, but were of immense practical interest to every engineer engaged in the use of steel in railways, steamboats, and the like. Although the theories advanced might not be considered entirely satisfactory, every engineer would feel deeply indebted to those physicists and metallurgists who gave so much time and thought in throwing light upon obscure subjects of such great importance. He had therefore great pleasure in proposing a vote of thanks to the Authors of the Papers.

Mr. THOMAS ANDREWS congratulated Prof. Arnold on the results of the important work described in his Paper. The chemical evidence as to the mode of existence of carbon in normal, annealed and hardened steels appeared conclusive. The mechanical tests were in consonance with the results of his own experiments on the influence of carbon on the physical properties of iron, on the relative corrosion, the passive state, and the heat dilatation of iron and steel. The microscopical evidence was also of the highest value. He had made an independent series of experiments on this subject, the results of which fully accorded with Prof. Arnold's observations. He could also support the view that "the laws determining the structure of iron containing various percentages of carbon are fixed and concordant for given physical conditions." The physical and magnetic portions of the research afforded confirmatory testimony to the accuracy of the other parts of the investigation, which clearly indicated that carbon in various states of combination, not solution, was the main factor in producing the hardness of steel; and that therefore it was unnecessary to revert to the theory of an allotropic change in iron for an explanation of phenomena connected with either the hardness or hardening of steel. Mr. Andrews.

The allotropic theory of metallic solution of one metal or element in another at high temperatures, or indeed at low temperatures, alleged that carbon, *per se*, pure and uncombined, was capable of being held in solution at high temperatures (about 700° C.) in pure molten iron without a resultant chemical combination having taken place. It was further maintained that on heating unhardened steel to a temperature slightly above 700° C., a dissociation of the iron and carbon occurred, heat being absorbed at the recalescence point, A_R1, and "that the atoms of carbon were then in a free state, not as graphite, but merely dissolved (not, however, as a chemical combination of iron and carbon) in the mass of iron atoms." This theory appeared scarcely conceivable in view of the fact that carbon, pure and uncombined, had a far higher melting-point than iron. If chemical combination were admitted the case would be different. The only solution of the difficulty would be the assumption that carbon under such high temperature conditions was capable of an allotropic modification, in which state it might have a lower melting-point. There had, however, hitherto been no practical demonstration of such phenomena occurring with carbon in molten iron. The thermal critical points on the cooling of steel, on which the β iron theory relied as indicative of allotropic change, should be regarded rather as

Mr. Andrews. indications of the separate periods at which the solidification or crystallization of the varied compounds of steel occurred. Thus, on the cooling of a mass of steel, the first recalescence point, A_R3, which took place at a temperature of about 880° C., would appear to indicate the commencement of the plastic condition of the primary crystals of the pure iron or ferrite portions of the steel. The segregated carbide of iron areas, having a lower fusion point, would not solidify until a lower temperature was reached. The critical point A_R2, which occurred at a temperature of about 740° C., would seem to be connected with the final solidification of the pure iron crystals, and also with the sub-crystallization of the secondary iron crystals, which were found within the periphery of the primary crystals of ferrite. This critical point in iron at 740° C. might therefore be regarded as the period of final solidification of the pure iron crystals. The critical point, A_R1, which occurred in steel at a temperature of 680° C., appeared to be intimately connected with and indicative of the formation and crystallization of the normal carbide of iron areas which, being mechanically dispersed throughout the ferrite, and possessing a lower fusion point, undoubtedly solidified or crystallized at a lower temperature than the ferrite or pure iron portions of the steels. This was indicated by the fact that the fusion point of pure iron was much higher than that of iron containing any trace of combined carbon.[1] The recalescence critical points observed by Mr. Osmond appeared therefore to be more intimately connected with the separate crystallizations of the constituents of steel, occurring at different periods during the cooling of the metal, rather than with any allotropic modification of the iron. This explanation was consistent with the indications of the thermal recalescence tests.[2] His views were further confirmed by his recent microscopical observation of a system of sub-crystallization which occurred on the cooling of large masses of iron.[3] The β iron theory appeared to be superfluous and an unnecessary addition to any explanation of the hardness and hardening of steel, as had been also shown by Mr. Howe, who had

[1] "The fusibility increases with the carbon, probably without limit."—"Metallurgy of Steel," vol. i., by Henry M. Howe, M.A., p. 17, § 32.

[2] See the illustrative curves, "An Introduction to the Study of Metallurgy," by Prof. W. C. Roberts-Austen, C.B., F.R.S., p. 107, and also the recalescence curves in "The Influence of Carbon on the Physical Properties of Iron," by Prof. J. O. Arnold, F.C.S., Journal of the Iron and Steel Institute, vol. xlv. 1894, Plates xv.–xvii.

[3] See "Micro-Metallography of Iron," Part I, by Thos. Andrews, Proceedings of the Royal Society, vol. lviii. p. 59.

remarked:[1]—"To sum up, Mr. Osmond's theory accords neither with the old nor his new facts, while the latter, like the former, harmonize well with the carbon theory. The carbon change being a fact, the $\alpha\beta$ allotropic change in iron, as yet wholly unproved, the balance of present probability was readily seen." Mr. Andrews.

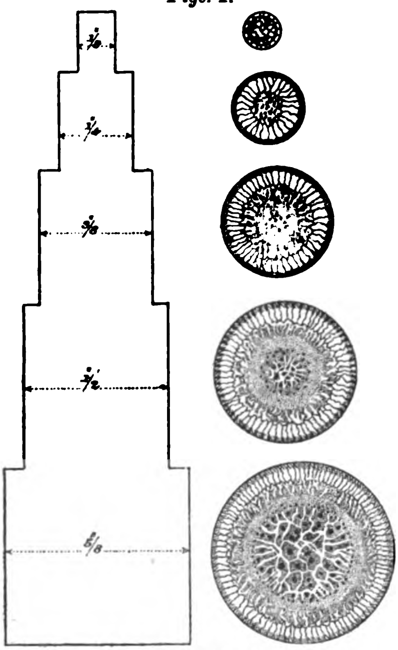
The investigation of Prof. Arnold had been conducted on a more extensive scale than had previously been attempted, and the consequent unity of results arrived at by his various correlative experiments was invaluable in connection with a correct apprehension of the influence of carbon on iron. On the other hand, experiments in support of the allotropic theory appeared hitherto to have been conducted on comparatively small masses of metal, the latter being inadequate for the purpose of exact observation; and they afforded but little comparison with the phenomena occurring on the large scale of practical experience. His own observations, both chemical, physical and microscopical, on the structure of iron and steel precluded his accepting the conclusions of the allotropic theory as to the causes producing the hardness and hardening of steel. In Sheffield, which might well be called the birthplace and natural home of the steel trade, and where probably the largest masses of steel were daily experimentally examined and manipulated, metallurgists were united in declining, without further demonstrable practical evidence, the β theory of the hardness and hardening of steel.

Mr. J. E. Stead had conducted a series of experiments continuing in a certain direction the work of Prof. Arnold, whose results they had confirmed in many cases. The iron and carbon compound, or steel containing 0·89 per cent. of carbon, he had found to consist of a perfectly pearly constituent. When the carbon was increased to 1·25 per cent. and then to 1·50 per cent., he had obtained the same separation of cementite as Prof. Arnold; but in certain annealed samples it assumed an entirely different form. On cross-sectioning and polishing the bars, instead of having the mesh-like appearance, the cementite presented small bright points. On sectioning the bar longitudinally those points proved to be the ends of continuous lines of cementite. Probably the reason for the different manner in which cementite formed was the difference in the heating and annealing processes to which the samples had been subjected. He had followed the matter in the direction of chemico-microscopic examination, and had commenced with a round bar, *Figs. 1*, of white iron, $\frac{1}{8}$ inch in diameter at one end and $\frac{5}{8}$ inch at the other, Mr. Stead.

[1] "Metallurgy of Steel," vol. i. p. 192.

Mr. Stead. subjected to a long-continued annealing process in oxide of iron. His object had been partially to remove the carbon from the metal, and get material in one piece containing gradually decreasing quantities of carbon. On sectioning and examining the sections, $\frac{5}{8}$ inch, $\frac{1}{2}$ inch, $\frac{3}{8}$ inch, $\frac{1}{4}$ inch and $\frac{1}{8}$ inch in diameter, under the microscope, all the constituents had been presented as described by Prof. Arnold. First, there was on the outside pure iron, almost free from carbon, which contained also oxidised silicon. Then occurred an area of iron, and then what he called the transition area, in which the iron was mixed with the pearly constituents, corresponding to Figs. 5, 6 and 7, Plate 4. A little further on there was an area which was purely pearly, corresponding to Prof. Arnold's Fig. 8; and still further a point which was similar to Fig. 10, in which there were large meshes of cementite intermixed with pearlite and graphite. With a view to finding the composition of those various layers of iron and steel, he had taken a portion of the casting, $\frac{1}{2}$ inch in diameter, and in a lathe had cut from it layers about $\frac{1}{100}$ inch at a time along the piece, and analyzed each layer carefully for combined carbon. Up to the point where the black patches began to appear, the carbon was of constant quantity, 0·02 per cent. That had astonished him, because the opinion had always been held that when solid steel was partially decarburized by an oxidizing agent from without, there was least carbon on the outside, the amount gradually increasing as the centre was approached; but it was not so. There was a large area of pure iron, and the crystals, strange to say, all

Figs. 1.



Outer zone . . . Iron and oxidized silicon.
Second ,, . . . Columnar crystals of iron.
Third ,, . . . Pearlite and iron in patches.
Fourth ,, . . . ,, and temper graphite.
Fifth ,, (centre) Cementite, pearlite and temper graphite.

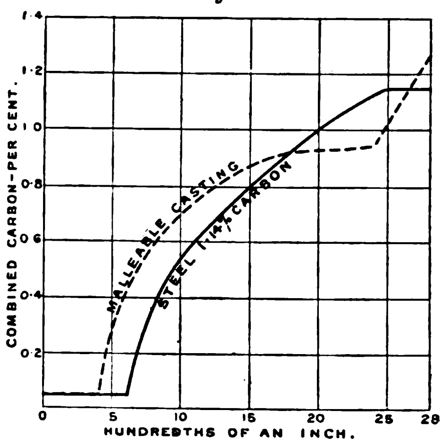
The fifth zone is absent in the $\frac{3}{8}$-inch section.
,, fourth ,, ,, $\frac{1}{4}$,,
,, third ,, ,, $\frac{1}{8}$,,

MICRO-STRUCTURE OF A MALLEABLE CASTING.

radiated towards the centre. At the end of that area, the next Mr. Stead.
step of $\frac{1}{100}$ inch showed a decided amount of combined carbon, and at each step after that the carbon steadily increased till reaching the centre area, which contained a mixture of graphitic temper carbon, cementite and pearlite. Near to the centre of the dark area the carbon was found to be about 0·90 per cent., and at this point the material exhibited the most perfect pearly appearance. On the area between this point and that in which the cementite was clearly visible there was present a considerable amount of graphitic temper carbon. This area contained between 0·8 per cent. and 0·9 per cent. of combined carbon, from which it would appear that this graphite acted as a reservoir of carbon, which was drawn upon as the removal of that element from the outside of the bar progressed, showing a tendency to maintain a considerable area with 0·9 per cent. of carbon.

It had occurred to him that the practically stationary period in which the carbon remained, strengthened the theory that in steel itself there was a tendency for a certain definite compound to form containing carbon with about 0·9 per cent. of carbon. On annealing ore steels containing 1·25 per cent. and 1·4 per cent. of carbon, the dark areas were also developed on polishing and etching; and the carbon in these areas gradually increased from the outside of the dark areas to the interior without any irregularity. The effect of ore annealing cast iron and steel was shown comparatively in *Fig. 2.* In the course of his experiments he had found it difficult by the microscope to find much apparent difference in the appearance of steels containing between 0·8 per cent. and 1·0 per cent. of carbon, but in all probability with more delicate apparatus it might be possible to do so. It would certainly appear that Prof. Arnold's various experimental data tended to show that the compound $Fe_{24}C$ was a normal constituent of hardened steel containing about 0·9 per cent. and above of carbon, but he did

Fig. 2.



Mr. Stead not agree that with less carbon than 0·89 per cent. the carbon still existed as $Fe_{24}C$ in steel when hardened. In fact Fig. 21, Plate 4, showed very much greater dark areas than Figs. 6 and 12, representing the steel before hardening. In the annealed steel 0·38 per cent. of carbon was sufficient to convert 40 per cent. of the mass into $Fe_{24}C$, but in the hardened sample of the same material it would be noticed that the dark areas were much greater than 40 per cent. It would appear that the carbon really extended beyond the areas containing 0·89 per cent. of carbon, and that therefore when sufficient iron was present $Fe_{24}C$ could not exist but was decomposed, and that a more basic carbide was formed containing more atoms than twenty-four in the molecule. He believed Mr. Osmond had concluded that what really took place was, as the steel was heated the carbon combined with a larger and larger amount of iron until the whole mass became permeated with carbon.

With reference to Mr. Wrightson's Paper, he had seen many of the experiments described, but could not exactly follow the Author with regard to expansion in the setting of white iron. He would ask whether it was not possible that when the cap was removed from the upper part of the balls, when the metal was still in a slightly plastic condition, the weight of the exposed portion would not bulge out the sides and thus give a misleading observation. He would ask also whether the vertical diameter of the ball was taken after the experiments to ascertain whether there had been any falling or sinking in a vertical direction. It seemed to him that a white ball should not expand more than a grey ball. The density of grey iron was about 8 per cent. less than that of white iron, although they were the same when in a fluid state. The reason was that a considerable amount of carbon separated out in the graphitic form in the grey iron, causing the mixture of the two separate bodies to be greater than the original combined carbon and iron. It would be naturally imagined that if the iron was being dissociated at the time of setting, and the carbon separated from it, the grey ball ought to expand more than the white ball, in which there was no dissociation. He could understand how it was that they both floated in molten metal, and that the white ball sank further than the grey. He should imagine that a more gradual curve up to the highest point would be obtained for white than for grey iron.

Prof. Roberts-Austen. Prof. Roberts-Austen said, with regard to Prof. Arnold's Paper, that it was with great pleasure he was able to remove what must have been for some years a misconception of the

Prof. Roberts-Austen.

Author respecting his views as to the hardening of steel. It had been stated by Prof. Arnold (p. 129) that "If it can be proved that at high temperatures the carbon (in steel) still remains in combination with the iron, the foundation of β (allotropic) iron theory will be destroyed." He was glad to be able to state that this was not the case. The main point that Mr. Osmond and he had so long maintained was that steel owed its property of being hardened mainly to allotropy of iron; that was, the iron might, as the later work showed, exist in three distinct molecular forms, called respectively α, β and γ iron. It was nevertheless recognized that carbon was a very important factor in the question, although they held that the iron in hardened steel was not molecularly the same as that which existed in soft steel. As proving that the allotropic theory of hardening steel would not be destroyed by evidence which would show that carbon remains in combination with iron at a high temperature, it might be pointed out that Mr. Howe, of Boston, had read a Paper[1] recently to sustain what he called the carbo-allotropic theory, a theory with which Mr. Osmond and he (Professor Roberts-Austen) had much sympathy.

The remaining points of difference between the Author and himself were rather those of detail, and he would be quite content to say no more were it not for the fact that the Paper controverted, not for the first time, views which had been maintained by Mr. Osmond, whose opinions he entirely shared. The main points at issue were:—Does carbon exist in the dissolved form in hard steel or as a carbide $Fe_{24}C$? or, if a carbide does exist, is it $Fe_{24}C$ or Fe_3C? It was, however, important that the real cause of the hardening of steel should be accurately known, quite apart from the theoretical interest of the question, because it would determine the course to be adopted in the thermal treatment of steel in its industrial applications. If a piece of steel was allowed to cool slowly, on becoming dull red, that was, at a temperature of about 650° C., it suddenly glowed again and became a brighter red. That effect was called "recalescence," and was a very critical point in the life history of the piece of steel. It was in explaining its cause that the difference occurred between Prof. Arnold on the one hand, and Mr. Osmond and himself on the other. "Recalescence" was held by Prof. Arnold to be due to the splitting up of a hypothetical carbide with the very improbable formula $Fe_{24}C$; and by Mr. Osmond to the fact that carbon, which was previously

[1] Journal of the Iron and Steel Institute, vol. xlviii., 1895, p. 258.

Prof. Roberts-Austen.

in solution in the iron, united with it and formed the carbide, Fe_3C, which Sir Frederick Abel had long ago actually isolated and produced from steel which had been slowly cooled. The existence of $Fe_{24}C$, on the other hand, rested on a theoretical basis, for it had never been isolated.

It had also been stated by Prof. Arnold that $Fe_{24}C$ was the cause of the hardness of hardened steel. While fully admitting the importance of the action of carbon in the hardening of steel, Mr. Osmond and he considered it was not the only cause, and that the hardness of steel depended on an allotropic change in the iron itself. They considered there was more than one kind of iron, just as there was more than one kind of sulphur, the plastic and the crystalline. The analogy was, in fact, very close between the behaviour of iron and of sulphur. They maintained there was no necessity to appeal to the existence of the carbide $Fe_{24}C$; and they could show that all the facts observed were in accordance with the laws of solution and chemical equilibrium; the solution of carbon in iron being, however, in some cases a solid one.

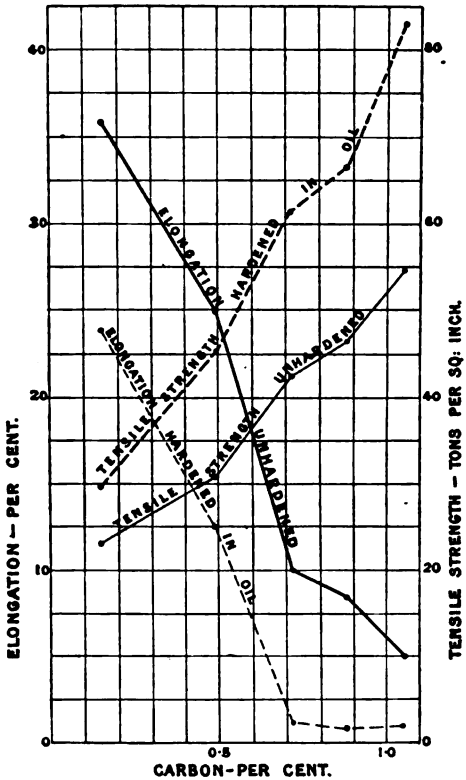
In the absence of any satisfactory chemical evidence as to the existence of such a compound, Prof. Arnold argued that all the physical properties of steel culminated when 0·89 per cent. of carbon was present; this amount of carbon required an amount of iron which would give the formula $Fe_{24}C$; therefore $Fe_{24}C$ was the cause of the hardening of steel. It was thought by Prof. Arnold that the properties of steel containing 0·89 per cent. of carbon were sharply distinguished from the properties of steel with other degrees of carburization, but had nowhere demonstrated the fact, nor offered chemical evidence that could be accepted in favour of the existence of such a compound as $Fe_{24}C$. None of the statements in the Paper were inconsistent with the fact that carbon existed in the dissolved form in steel at a high temperature, and Mr. Osmond and Prof. Roberts-Austen further stated that in very hot steel there was true solution of carbon in the iron; and that following the laws of solution the saturation point might correspond with some 0·9 per cent. of carbon. Further, a new formula for a supposed carbide would be found at each new temperature to which the steel was heated and from which the steel was quenched.

None of the mechanical properties Prof. Arnold had adduced were inconsistent with the fact that the saturation point of iron by carbon at a definite temperature was within the limits of 0·8 per cent. to 1·0 per cent. of carbon. The tenacity of the series of steels in the normal condition, *Fig. 1* (p. 133), turned with 1·20 per cent. of carbon,

which would correspond nearly to the very improbable formula $Fe_{18}C$. The ductility, *Fig. 2* (p. 133), of the normal steel series appeared to turn, if there be a point there, with about 0·59 per cent. of carbon, which would correspond to a carbide $Fe_{36}C$. The formula $Fe_{24}C$ demanded that all the mechanical properties of steel should turn at 0·89 per cent. of carbon. Comparing *Fig. 2*, p. 133, with the subjoined diagram, *Fig. 3*, he had long used in his class teaching at the Royal School of Mines, it would be seen that certain properties did turn at about that percentage, but surely the range between 0·75 per cent. and 1·25 per cent. should have been explored more rigorously than was shown in *Fig. 2*, p. 133, before basing any conclusions upon the results there given. The annealed steels, however, should not be appealed to at all, because those which contained more than 0·89 per cent. of carbon also contained the one 0·28 per cent. and the other 1·14 per cent. of graphite. Saturation of the solution was complete at about 0·89 per cent., and then graphite fell out. As regarded the compression tests on which Prof. Arnold specially relied, *Fig. 3*, p. 135, all the specimens seemed to have been subjected to stresses beyond their limits of elasticity, and therefore they were valueless as affording evidence of molecular structure. He would much like to hear Dr. Kennedy's opinion as to these curves and the way they were plotted. He dissented from the view that under these, or indeed any circumstance, "compression tests furnish by far the best means of

Prof. Roberts-Austen.

Fig. 3.



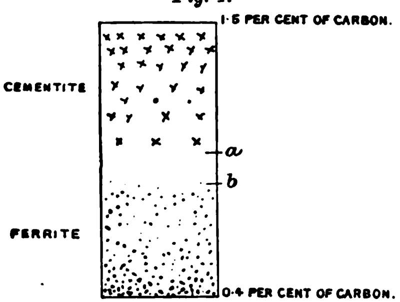
Prof. Roberts-Austen.

measuring the molecular rigidity of metals," as friction came into play to so great an extent.

In dealing with the microscopical section of the work, he was in some difficulty because Prof. Arnold had placed himself at a great disadvantage by resorting to hand-drawing the micro-sections, the excellence of which, however, as drawings might gladly be admitted, instead of adopting the well-known photographic method which, as the specimens placed on the table showed, admitted of such splendid fidelity in rendering the results. When viewed side by side with Mr. Osmond's photographs of micro-sections, Prof. Arnold's method made his work seem strangely retrograde.

In a section of steel carburized from one end, the amount of carbon varying continuously through the thickness of the mass, between, say, 1·5 per cent. at the top and 0·4 per cent. at the bottom, there should, if Prof. Arnold's view were correct, be a sharp line of demarcation in the microscopic structure where the carburization was 0·89 per cent. But this was not the case. There was really a comparatively wide range of carburization, *a b*, *Fig.* 4, which presented exactly the same micro-structure as was claimed by Prof. Arnold exclusively for steel which contained 0·89 per cent. of carbon.

Fig. 4.



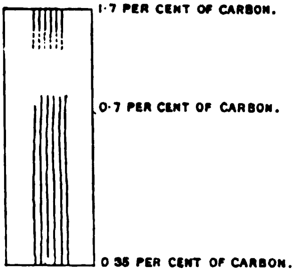
The portion which contained neither isolated ferrite (pure iron) nor isolated cementite (Fe_3C) was not sharply defined as his theory demanded. To sustain the view that the micro-section of hard steel containing the 0·89 per cent. of carbon, demanded by the formula $Fe_{24}C$, was widely different from the micro-section of hard steel which contained other percentages of carbon, why had the one piece of evidence which would have confirmed or demolished his theory not been given? Why had drawings (not even to ask for photographs) of steel No. 3 and of steel No. 5 in their hardened state not been shown? He was "instructed" (for this was a point on which he could not speak as an authority), that if Prof. Arnold had given drawings of steel Nos. 3 and 5 in the hard state, they would have corresponded with the illustration he had given of steel No. 4, which contained 0·89 per cent. of carbon, and the correspondence of Nos. 3 and 5 with No. 4 would,

Prof. Roberts-Austen.

so far as microscopical evidence was concerned, not have supported his theory.

The view that iron was an allotropic body had always derived much support from the behaviour of manganese steel. Steel containing a certain percentage of manganese was not magnetic. Red-hot iron was not magnetic, therefore it was said the iron was probably present in red-hot iron in the same form as it was in hard non-magnetic manganese steel. With reference to that point, Prof. Arnold practically said, "if the allotropists are right, quenched carbon-steel should be non-magnetic, which is absurd"; and he added that "between quenched carbon steel and manganese steels there is not the slightest magnetic analogy." As there was no supplemental note to the Paper with reference to this last point, Prof. Roberts-Austen supposed the Author had not seen the account of some work recently published.[1] He would therefore refer again to the diagram, *Fig. 4*, p. 139, to which reference had just been made. If a bar of steel be carburized by cementation, from one end in which the amount of carbon varied between 1·7 per cent. and 0·35 per cent. of carbon, and heated strongly and subjected to very rapid cooling in ice-cold water, so as to harden it energetically, and then ruled with fine lines by the aid of a hard needle, *Fig. 5*, it would be found that, as might be expected, those lines which were clear in the softest part disappeared in the hardest part—that was, where the percentage of carbon was about 0·7 per cent.; but, contrary to all preconceived opinions, it would be found that these reappeared in the upper and more carburized part. If steel of the degree of carburization indicated by that critical portion, viz., with about 1·5 per cent. of carbon, be heated to about 1,100° C., and quenched in ice-water or very cold mercury, a variety of steel would be obtained which was comparatively non-magnetic, and showed, under the microscope, the presence of two constituents, A and B. The determinations of the residual magnetism showed that the intensities of magnetization of bars quenched, at a temperature of 1,000° C., in ice-cold water were 345 immediately after magnetization, and 221 two

Fig. 5.



[1] "Sur la trempe des aciers extra-durs," Note of Mr. F. Osmond, presented by Mr. Troost, Comptes Rendus, vol. cxxi., 11 November, 1895, p. 684.

Prof. Roberts-Austen. days after magnetization; and of bars quenched, at a temperature of 800° C., in water at 15° C., 966 and 814 respectively. There were in that bar two constituents, A was magnetic and polar, and the conclusion was led to that B was not magnetic, and the properties of the constituent B were thus directly comparable with the properties of steel containing 25 per cent. of nickel or 12–13 per cent. of manganese, and thus a point of great importance to the theory of Mr. Osmond was established.

The case might be put thus. There was no doubt, he thought from his own experiments, that the maximum "recalescence" did occur in steel which contained some 0·9 to 1·0 per cent. of carbon. The explanations of Prof. Arnold required an assumption given on p. 150, which he very justly said was somewhat startling. Allotropists gave an explanation which presented a simple case of chemical equilibrium between carbon in solution and iron and carbon in combination, a theory which had every probability in its favour. The solution theory did not demand, as Prof. Arnold thought, that the line, *Fig. 25*, p. 152, should continue straight. It turned naturally at the saturation point of iron with carbon, that was, at the temperature of recalescence with about 0·9 to 1·0 per cent. of carbon.

In regard to the Paper as a whole, the Author had called one section of it "Historical," but it was useless to give a series of names and call it history. If the authorities whose names had been cited could be quoted, it would be seen that their investigations contained information which would have enabled him to build up the theory he had advocated without a new and laborious research. If it had been his own work which had been attacked, Prof. Roberts-Austen would have said nothing, because he was sure that theories would be accepted or rejected in the ordinary course of research without the need for attack or defence, but he entirely differed from the conclusions, although he warmly appreciated the experimental part of the work, and he felt he would be wanting in respect for the Institution if he allowed this appreciation of the manipulation to prevent him from expressing clearly the opinion that the Paper did not do much to advance the theory of the nature of steel.

As to Mr. Wrightson's Paper, he was glad that such excellent work had been done in the Mint Laboratory, and he could testify to the extreme care with which the experiments had been conducted. It was a great thing to have established a close relation between the regelation of ice and the welding of steel.

Mr. R. A. HADFIELD desired to congratulate Prof. Arnold upon the result of his researches, which had involved so many years of patient work. The facts which he had accumulated and stated in the Paper seemed to afford the most satisfactory explanation of the singular phenomena noticed in connection with the hardening of carbon steel. The views expressed confirmed his own opinion, that it was only by study of the behaviour of carbon in its various combinations that a satisfactory explanation of the changes induced by heat treatment and cooling of various kinds of steel must be looked for. It was difficult to understand how the β iron theory could be sustained in face of this further valuable evidence; and he was therefore more than ever averse to the adoption of that theory as affording a satisfactory explanation. The carbon theory, as Mr. Howe had rightly termed it, was now confirmed by additional facts which could not be explained away, and were borne out by the observations of ordinary practice. Mr. Hadfield.

He considered the existence of a definite carbide of iron a satisfactory explanation of the facts noticed in connection with the hardening of carbon steel. The extraordinary results obtained in hardened steel were, no doubt, exceedingly complex; but if definite compounds could be shown to exist, it should be accepted that chemical compounds must almost necessarily present entirely different properties to those of their original elements, without recourse to allotropic and other theories. The remarkable fact mentioned in the Paper, that it was possible to detect by the microscope such small quantities as 0·08 per cent. of carbon in definite chemical combination, afforded an important proof of the influence of small quantities of this element, which had often been overlooked, notwithstanding the excellent work of Professor Roberts-Austen. The effects of cold working were being adduced as proof of the existence of allotropic modifications of iron, in other words, of the existence of β or hard iron. But this remarkable fact, pointed out by Prof. Arnold, in conjunction with the extraordinary susceptibility of steel to gradual, almost imperceptible, changes of the carbon or carbides present, at temperatures hitherto considered to be of small importance, combined with those noticed in connection with ordinary structural changes induced by heat treatment, should make the effects of hardening by wire-drawing or other similar treatment readily understood. It would seem, therefore, that recourse was unnecessary to theories the facts for proving which have never yet been adduced. No specimen of β iron had ever been isolated or detected, and unless some more definite evidence was forthcoming from the supporters of that

Mr. Hadfield. theory, in face of the remarkable array of facts in the Paper, and those he had observed in the behaviour of certain alloys of manganese steel,[1] it would seem useless to pursue the investigations.

As regarded the excellent designation "saturation point," about 0·89 per cent. of carbon, proposed by Prof. Arnold, to indicate the most marked and apparent chemical combination of iron and carbon, he could confirm the view from observation extending over many years of the behaviour of a large number of varieties of steel. He had, several years ago, been convinced that the addition of carbon beyond a certain point, in order to obtain increased hardness, was in itself an error. There were certain varieties of steel, for example, that used in the manufacture of armour-piercing projectiles, in which nearly all makers had, although each was unaware of the others' practice, arrived at almost the exact percentage of carbon mentioned by the Author as that which would give the best results for toughness and hardness. The effect of other elements upon carbon steel had been to make this saturation point more apparent; the reason being thus afforded for most alloy steels having stopped at percentages of carbon, which for hard steel had hitherto been considered low. It had not been found necessary for such steel to contain more than about 0·8 per cent. of carbon, which was exactly the percentage which Prof. Arnold had designated "saturation point," a most convenient and suitable term. The foreign elements present caused the special carbide to act in the main more strongly, and a smaller quantity of carbon would give hardness. This was proved by the fact that if the carbon be taken away in such alloys the products did not differ widely from one of pure iron.[2]

He was gratified to find that Prof. Arnold continued to strongly confirm his own belief, that the peculiar behaviour of manganese steel, which by heat treatment he had been able to make magnetic or non-magnetic, largely depended upon the particular form of carbon present. The clear proof given by Mr. J. E. Stead in the discussion[3] on his Paper, "The Results of Heat Treatment on Manganese Steel and their bearing upon Carbon Steel," seemed to leave no doubt upon this point; but with the further evidence available he thought it could not be questioned. He felt convinced

[1] "The Results of Heat Treatment on Manganese Steel and their bearing upon Carbon Steel," and discussion of the Paper. Journal of the Iron and Steel Institute, vol. xlv. p. 156.

[2] "The Physical Influence of Elements on Iron," by Professor J. O. Arnold, Journal of the Iron and Steel Institute, vol. xlv. p. 107.

[3] *Ibid*, vol. xlv. p. 191.

that the evidence in the Paper proved that it was only by study of the action of carbon, in the form of different carbides, that a true solution of the important and complex question of the hardening of carbon steel could be found. Mr. Hadfield.

For many years he had been endeavouring to obtain an iron combination that would be naturally hard without the intervention of quenching or rapid cooling. White cast-iron was such an example, but as this product contained between 3 per cent. and 4 per cent. of carbon its hardness seemed to have caused no surprise. The explanation generally adopted was that such hardness was due to the existence of a carbide of iron. But there were metallurgists who, when they came to examine steel containing say 1 per cent. of carbon, were not satisfied with the foregoing, and so the subject had been overwhelmed with theories of the most complex nature. Whatever might be the difference of opinion as to Prof. Arnold's Paper it was certainly a true attempt to give a practical solution of this question. It seemed to throw light upon facts that occurred in practice which was not the case with other theories that had been advanced. It was for this reason that he had, for the last five or six years, been anxious to obtain an alloy or combination of iron and carbon, and, if necessary, one other element, that should possess the following qualities: (1) It should be comparatively low in carbon, say about 0·50 per cent., so as to in no way resemble high-carbon compounds, such as the white cast-iron referred to; (2) It should be a steel, that is to say, malleable at a red heat; (3) It should be naturally hard, either in the cast or forged state, that is, hard without the further treatment such as was generally used, quenching in water or other cooling medium or rapid cooling in air; (4) The hardness should be similar to hardened carbon steel, that was, sufficiently hard either in its cast or forged state to scratch glass; and (5) The most important of all, that the same alloy without carbon should possess exactly the opposite qualities as regards hardness; that was, in the absence of carbon the material should be soft and incapable of being hardened in any way. Such a product, or two such products, would clearly prove that the lines of future research must go in the direction which, like himself, Prof. Arnold so strongly urged, viz., the study of the carbides of iron. It was one thing to compose mentally such alloys, but quite another matter to produce them. It was not of course difficult to produce iron alloys with carbon, nor was it difficult to obtain carbonless iron alloys in combination with aluminium, silicon, sulphur, phosphorus, nickel, cobalt, copper or even chromium;

Mr. Hadfield. but none of these products were hard (that was to say, possessed glass-scratching hardness), either in the absence of carbon or with the carbon increased even to comparatively high percentages. There was still one element left, manganese, which in iron alloys and with about 0·50 per cent. of carbon met all the requirements he had given, but in itself it proved nothing decisive, for its corresponding carbonless alloy without the carbon had not before been obtainable. The qualities of the iron alloy with carbon and manganese, had been fully described in his Papers[1] communicated to the Institution. As regards the production of the carbonless alloy, he had tried to decarbonize the ferro-manganese or spiegel, used to produce the alloy, also to decarbonize the steel so made, but unsuccessfully. As no carbonless ferro-manganese, or manganese metal, was obtainable, the matter had had to rest, after very many attempts during five or six years, and it was not until quite recently that he had been able to obtain an alloy from America containing 93 per cent. of manganese and practically no carbon. It was with no little satisfaction that he then found it possible to produce the long-thought-of theoretical alloys, which he had now the pleasure of showing, and which, in his opinion, most substantially confirmed Prof. Arnold's theory as to the existence of a definite hard carbide of iron. He held in his hand two samples, made precisely similar. The one was an alloy containing about 3·5 per cent. of manganese, and less than 0·1 per cent. of carbon. That was easily drilled, there was no difficulty whatever in machining it, and it could be easily filed. The other sample gave precisely the same analysis as regards manganese, but the carbon was about 0·54 per cent. That was a remarkable alloy, so hard that not only could it not be filed, but it could not be machined in any way. Here were two products having almost the same analysis, in which so comparatively small a difference as 0·40 per cent. of carbon changed a soft iron into an alloy somewhat resembling hardened carbon steel; it was very remarkable, and he was very glad to have the opportunity of placing it before the Institution, as these alloys had only been made a few days.

On the particular point Prof. Roberts-Austen had emphasised, in a Paper read before the Institution some years ago, he (Mr. Hadfield) had suggested similar explanations to those taken up by Prof. Roberts-Austen and Mr. Osmond. The light of further evidence had, however, in his opinion, shown that it was not the true direction in

[1] Minutes of Proceedings Inst. C.E., vol. xciii. pp. 1 and 61.

which to look for a correct explanation of the hardening of iron. It was, no doubt, a very difficult matter to understand how it was possible to produce a hard iron alloy without some such explanation as that of an allotropic change; but these two samples appeared most decisively to prove that unless carbon was present there was no hardness; in other words, take away the carbon, and there was no hard form remaining. It, therefore, did not seem necessary to call in the aid of allotropy. It had been complained by Prof. Roberts-Austen that his theory also depended upon the presence of carbon; but that was exceedingly difficult to follow in a practical way. He could only examine the alloys with the respective elements present, and the hardness could hardly be put down to anything but the carbon. His own view was that in such an alloy as 3·50 per cent. manganese, and 0·5 carbon, the carbon acted in a very much stronger manner, and consequently, an alloy in some respects resembling hardened steel of much higher percentage of carbon was obtained. In face of those specimens, he could not see how it was possible to doubt the existence of definite combinations either as single, or in this case double carbides of iron and manganese. He did not put those alloys forward as having any practical value. In fact, to give some idea of the brittleness of the one with carbon, he produced a small sample which had been reduced to fine powder under an ordinary hand-hammer. It seemed extraordinary to find such a great difference on merely increasing the carbon from about 0·1 per cent. to 0·6 per cent. He thought that the Paper would considerably help to some satisfactory solution of the important questions dealt with. It was ably and clearly shown by Prof. Arnold, not only theoretically but practically, on the lines so well dealt with by Sir Frederick Abel, that it was in this direction metallurgists must now direct their attention. Mr Hadfield.

Mr. JEREMIAH HEAD did not propose to say much on the subject under discussion, and certainly would not attempt, even if he were able, to enter into the question of the difference of opinion which seemed to exist amongst the real authorities in the matter. He might say, however, that he had had an opportunity of visiting the technical school at Sheffield, and he had been greatly struck with the wonderful opportunities there existing for carrying on the arduous work which Prof. Arnold had been undertaking. He thought it was much to the credit of the Sheffield Corporation that they should support such a school, which was of the greatest service to the country. He had found even in America men in responsible positions who owed much of their metallurgical knowledge to an education at that establishment. Mr. Head.

Mr. Head. Iron and steel makers could not find time to make the experiments necessary for original research, and that was equally true of the civil engineer. His vocation was rather to take the materials and forces of nature and to apply them for the benefit of man. But how was he to apply those materials unless he kept up his acquaintance with them, unless he knew their peculiarities and characteristics and everything new that was brought to light with regard to them. It was therefore of the utmost importance to have at the Institution Papers of the kind under discussion, in order that such things might be known about as fast as they were discovered. When, a great many years ago, steel had begun to supersede wrought-iron in metallic structures, it was known that wrought-iron was anything but a homogeneous material. It was composed of the metal iron combined with a great deal of a nondescript substance called cinder. An ordinary wrought-iron gun-barrel, which had been subjected to acid, gave as good an idea as anything could of the heterogeneous nature of wrought iron. But it had been supposed that when steel superseded wrought-iron a substance would be afforded that was perfectly homogeneous, and that was the reason why its physical properties were claimed to be so superior. It was somewhat startling to find, when the microscope was brought to bear upon those micro-sections, that the metal was almost as diverse in its appearance as the beautiful pictures produced by the kaleidoscope. The only two normal specimens which were at all homogeneous were that shown in *Fig. 4*, p. 139, which represented pure iron, and that shown in Fig. 8, Plate 4, which represented iron fully saturated with carbide. The first was a homogeneity, which meant moderate tenacity with high ductility, and consequently the maximum reliability, and the other was a homogeneity which meant high tenacity with low ductility, and consequently a constant tendency to treacherous behaviour. Those seemed to be the two extremes: but in the practical range of steel used for constructive purposes, Nos. 1 and 2 seemed to cover the whole ground. No. 1 was almost identical as regards the carbon it contained with very soft steel, the softest that he knew in general use, viz., that employed in America in the fire-boxes of locomotives. The main difference was that the latter had more manganese than appeared in No. 1. No. 2, with the same exception, resembled closely an analysis which had been recommended by Mr. E. Windsor Richards as the best composition for rails. That seemed to show that only a small portion of the range upon which Prof. Arnold had experimented was really any use for structural purposes. He was excluding for the

moment hard steels for tools and for special purposes. The qualities intermediate between Nos. 1 and 2, including the latter, were far from being homogeneous materials. He should be glad to know from Prof. Arnold if he thought the two ingredients, the pure iron and the carbide, might be expected to act together. Judging by the behaviour of the pure iron shown in *Fig. 4*, p. 139, and the saturated iron shown in Fig. 8, Plate 4, he thought that they would scarcely act together, and upon that the value of the material for structural purposes largely depended. A cable composed of elastic ropes and rigid chains twisted together would certainly not be a good one. When heavily strained the ropes would stretch and leave the chains to take the load. When the latter gave way, the ropes (so far but slightly stretched) would suddenly have the entire load thrown upon them and would give way at once, their extra ductility being of no real service. Might not the same be the case with heterogeneous steels such as No. 2? It was necessary, however, to bear in mind the smallness of the scale of those micro-sections. On the wrought-iron barrel which he had mentioned the heterogeneity was obvious without the aid of a microscope, but the micro-sections illustrated by the diagrams on the wall represented circles of only about $\frac{1}{16}$ inch in diameter. With regard to annealing, it would be noticed in Table IV that in the steels Nos. 1, $1\frac{1}{2}$ and 2, there was a serious reduction in the elastic limit, and also in the tensile strengths of the annealed samples, although the ductility shown by the extension and contraction was very little altered. That might possibly have an injurious effect in some cases, as where steel plates turned out of mills were sometimes laid on the floor several deep, which had practically the effect of annealing them; and it was possible that by such means accidentally their elastic limit and tensile strength might be seriously interfered with without its being at all suspected. Mr. Head.

With reference to the second Paper, he might mention that he had the pleasure of hearing the first Paper read by Mr. Wrightson about fifteen years ago; and he had been struck by the scientific and painstaking way in which he had dealt with his subject. He asked if the spherical ball was the best shape that could have been adopted for the casting. It was shown how the crest of the ball first hardened, and then a great many differential strains were set up in the interior and a considerable hollow was left at the top. He wished to know if some other form would not have avoided those difficulties and led to more certain results. The endeavour to identify the welding of iron with the regelation of ice was exceedingly interesting. Very strong evidence seemed

Mr. Head to have been produced in proof of the theory laid down, but the difficulties of making the arrangements and carrying them out were so great, that it was hard even now to regard the proof as absolutely conclusive. He would ask how the fact that in a bar of iron or steel the material at the weld was always weaker than that on each side of it could be accounted for. When ice was regelated was it also weaker at the point of regelation than on the two sides? It would be interesting if the experiment were carried further, and tried on platinum, which was another metal capable of being welded; and if it could be shown that the temperature of those metals that would not weld, such as copper, when pressed, increased instead of diminished. If the experiments could be continued to embrace those and other tests, it would be interesting and might assist in establishing the ingenious theory that Mr. Wrightson had enunciated.

Dr. Anderson. Dr. W. Anderson remarked that it was a great satisfaction and pleasure to find that the veil was being slowly lifted from the complicated question of the molecular structure of steel; and every research like the one so ably brought before the Institution, and even the replies which it had called forth from authorities like Prof. Roberts-Austen and Mr. Osmond would be of incalculable service. He wished to mention one somewhat remarkable instance of molecular action that had lately come under his notice, viz., the curious effect of electric welding on steel and iron. An electric welding-plant had been set up at the Royal Arsenal, and he would therefore be enabled to make some experiments to elucidate this matter. If a bar of iron were welded—a bar of fair size, say 1 inch or 1½ inch in diameter—by means of electricity, and if the weld was not afterwards hammered, the result was that the tenacity of the iron diminished to about that of cast-iron—it seemed to have its molecular structure entirely altered. But if the same bar were hammered immediately after welding, it resumed its natural qualities completely. The same effect had been observed in a bar simply heated to welding temperature. He had no doubt that Dr. Hopkinson would be enabled to explain that peculiar behaviour of the metal under the influence of electricity. The same thing happened in regard to steel, but not in so marked a way as with iron. The explanation he thought must be that which Mr. D. Chernoff had given[1] of the effect of forging on the molecular structure of steel,

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[1] "Remarks on the Manufacture of Steel, and the Mode of Working it," translated by Dr. Anderson. London: William Clowes and Sons, 1876.

viz., that the concussion and agitation caused by hammering had much the same effect on the structure of the mass as agitation had on a concentrated hot solution of alum allowed to cool while it was being agitated and stirred. If it were allowed to cool quietly, it would crystallise in large crystals; if it were kept agitated, it crystallised in very fine crystals. In the same way, if steel or iron under the influence of electricity were welded, and then allowed to cool quietly, the process appeared to favour the development of large crystals, and consequently of a brittle, worthless material. But if it was hammered immediately afterwards, the hammering prevented that, and caused the crystals to be small, and the substance to resume its normal nature. He had been told that this result was only arrived at when the temperature was allowed to rise too high, almost to the point of fusion; he could not answer for that, but no doubt further experience would throw some light on the matter. He did not quite understand the conclusion that Mr. Hadfield seemed to have arrived at, from the fact that there was such a small difference—between 0·1 per cent. of carbon and 0·54 per cent. of carbon—in the two specimens which he showed. He did not see that what he would call a large range in the contents of carbon quite overthrew Mr. Osmond's β theory of the hardness of steel, because he thought it was easily understood that where there were at least three elements present, the observed effect might arise from the action of carbon on manganese, just as much as carbon upon iron. The whole question was extremely difficult and obscure, and a great many more Papers of the same kind would have to be produced before it could be completely explained. Dr. Anderson.

Mr. B. Blount would confine himself to some of the matters respecting Mr. Wrightson's Paper. The suggestion by Mr. Head particularly appealed to him—that of investigating the question with the metal platinum, which was free from the vice of oxidation. In the case of iron, when two surfaces were brought together, a complex series of phenomena occurred. The iron was almost necessarily, for it was a difficult matter, as he knew from experience, to protect it from the air, exposed to oxidising influences. It was not a pure surface of iron which had to be dealt with, but one covered with a film of magnetic oxide; and, although doubtless the film was pushed aside, it was hard to say precisely what influence it might have in the cohesion of the boundary layers of the molecules of the two surfaces. Moreover, taking pieces of iron as pure as Prof. Arnold had succeeded in preparing, and placing them together, there Mr. Blount.

Mr. Blount. was nevertheless a considerable quantity of other elements than iron, and their influence was more or less unknown, so that recourse was naturally had to a substance absolutely homogeneous, and platinum was eminently such a metal. He feared that the welding of platinum in 2-inch bars scarcely came within the powers of ordinary experiment; but perhaps Mr. Matthey could undertake such an interesting experiment. Failing direct evidence on that point, humbler methods must be resorted to, and the actual results of experiments on iron and steel considered. There was one point that seemed of much interest—that when steel containing more than a certain percentage of carbon was welded by any means (he had particularly seen it in an electric welder), there was always a considerable risk of its structure being so altered that the tensile strength of the joint fell much below that of the original steel. That might be correlated intimately with certain facts set down in the first Paper. The bond between the two consisted in the circumstance that in both cases an alteration, not of molecular constitution, but of mere crystalline structure had to be considered. When two metals were united at a temperature considerably above the critical point of welding, which must be comparatively low, the union ceased to be a true welding and became more of the nature of fusion. In that fusion it might well occur that crystals of comparative magnitude and with separating surfaces by no means negligible, were formed, and such a crystalline mass could scarcely be expected to retain the tenacity and ductility of the original material. He thought that that tendency, and the fact that iron was not like water in the form of ice a homogeneous material, accounted for the well-known circumstance that welds were usually considerably weaker than the original material. Further, he thought that the facts described by Mr. Wrightson as to the flow of metal at the critical temperature of welding might be regarded as being intimately related to the classical researches of Spring, who had found that metals were capable of undergoing flow at temperatures much below their fusing point if the pressure was sufficient. The pressure which could be put upon a welding surface by hammering might be considerable, although no doubt it was local. A local flow would accordingly occur, so that, although a material might be sensibly below its melting-point, it might, and indeed did, weld. He should like to offer a small correction on a statement made by Mr. Head, viz., that copper would not weld. He did not know that that was quite correct. It might be taken that lead would weld; certainly clean surfaces pressed together

at sufficient pressure would actually coalesce, and he thought the same thing might be said of copper. Mr. Blount.

Dr. ALEX. B. W. KENNEDY had no intention of entering into the purely chemical controversy between allotropists and carbonists, but Prof. Arnold had raised a number of mechanical points on which he should like to say a few words. The first related to *Figs. 1*, *2* and *3*, pp. 133 and 135. He gathered from the mechanical results there set forth that the Author considered there was evidence of some special change occurring at about 0·9 per cent. of carbon. He submitted, however, that those diagrams by no means confirmed this. There was a discontinuity at 0·9 per cent. in a number of cases, but this appeared to be merely due to the accident that Prof. Arnold had taken that particular percentage as one of those at which he had made an experiment. It could hardly be doubted that the real curve ought to be more or less continuous—that it should be, in fact, a reasonably fair curve drawn through the observed points, and if that were done, he thought that in every case the curve would stand, a change occurring somewhere about 1·0 per cent. or 1·1 per cent. of carbon, instead of 0·9 per cent. This was the only point really indicated by the curves, and to justify the inference of the earlier change he thought that, in spite of the trouble it would have involved, Prof. Arnold ought to have given an experimental result actually at about 1·0 or 1·1 per cent., to justify his assumption that it fell on the arbitrary straight line, dotted or full, which had been drawn in the figures. A number of mechanical experiments, some in tension and some in compression, were also given in the Paper. As to tensile resistance and the elastic limit, the tensile resistance was of course all right and easily comparable. The elastic limit of a piece of iron or steel about ½ inch in diameter and 2 inches long was obviously only a yield point, not determined with any such accuracy as to be really a true limit of elasticity. The quantities given in compression were clearly measured entirely beyond the elastic limit, and after the material had become "plastic." Of course they represented certain observations; but he thought when steel attained the plastic condition of a cylinder which was actually squeezed down so as to be measurably much shorter and therefore much larger in diameter than before, that condition was so special that it could not teach much. What engineers would like to know, if it was not putting too great a burden upon the metallurgists, who had taken so much trouble already, was the influence of carbon on the elastic properties of the metal, because they endeavoured always to use it while it was really in an elastic Dr. Kennedy.

Dr. Kennedy. condition. Of course this was a very difficult result to arrive at. But he thought, in receiving the Author's figures to which he had just referred, it ought to be remembered that practically all of them referred to material in a condition in which it was not employed in practice. This was especially the case with the experiments in compression. In making this criticism he did not wish to detract from the value of a Paper containing such a mass of facts and observations as Prof. Arnold's; and what he had said might or might not have any bearing upon the controversy of which they had had some indication that evening, as well as on former occasions. Engineers were only too glad to know all they possibly could know from all competent sources, such as Prof. Arnold's experiments, as to the molecular condition and the other hidden mysteries of the materials which they had to use.

Prof. Arnold. Prof. ARNOLD, in reply, wished to suggest that the criticism of Prof. Roberts-Austen was a little inartistic, lacking middle tone. The high lights representing the work of his friend Mr. Osmond were so vivid, and the dark shadows showing the useless influence of the speaker's research were so deep, that he had a little overdone it. He thought such a statement as that, after five years' work and research, nothing had been added to knowledge of the nature of steel, was going rather too far. The opinion of practical steel metallurgists, who were constantly dealing with such questions, and who had had during several years full opportunity of testing them, had led them to an opposite conclusion. He might say that the β iron theory would not act in the workshop, and he had great pleasure in knowing from many competent observers that the sub-carbide theory, which he had the honour of laying before the Institution, would so work. It had been asked by Prof. Roberts-Austen why that sub-carbide of iron was not produced. He might, using the *tu quoque* argument, ask why also β iron was not produced. The sub-carbide of iron had been, and could be produced. That brought him to a point which had been referred to by Prof. Roberts-Austen, who had said that if he had read the historical portion of the research he would have obtained all the necessary information. He could not agree with him in that statement. Metallurgists knew well that the series of alloys referred to was the first series of that magnitude of practically pure iron and carbon steels that had been melted. Taking such a series, the difficulty of preparing which was well known, he had carried out mechanical tests, microscopical tests, and thermal and magnetic tests, and the comparative results obtained in those various methods of observations should certainly

be of some little use as bearing upon the problem in question. Prof. Arnold. It had been stated by Prof. Roberts-Austen that allotropists were aware of the saturation point of steel and that the maximum recalescence was presented at 0·89 per cent. of carbon. This statement was inaccurate, as would be seen from the following assertion by Mr. Osmond:[1]—"The station Ar1, which increases from extra mild steel up to the hardest steel, diminishes on the other hand on passing from hard steel to white pig iron." It was therefore evident that the allotropists expected the maximum heat at about 2 per cent. of carbon.

The micro-diagrams had been referred to by Prof. Roberts-Austen, who had expressed a regret that they had not been reproduced by photography. That meant, if it meant anything, that they were not quite accurate; he did not quite see the bearing of the remark if it did not mean that. He was, however, in the happy position that the microscopical portion of the research had been carefully confirmed by two observers of whom Prof. Roberts-Austen himself had spoken in high terms, Mr. Andrews and Mr. Stead, who had made perfectly independent sections. He had been congratulated upon the fidelity of the diagrams by Mr. Stead, who thought they reproduced the sections rather better than photographs on account of the difficulties of lighting. All the sections were on view at the Sheffield Technical School, and any engineer who had any doubt on the point could see them and compare them with the drawings that had been exhibited and could judge whether there was any justification for Prof. Roberts-Austen's complaint that the diagrams were not photographs. That Mr. Osmond had shown great skill in producing the best results capable of being figured by photography he fully admitted; nevertheless, the results were very misleading to the student. An inspection of Mr. Osmond's photographs taken at high powers would reveal the fact that most of them consisted of a large central white patch almost devoid of structure. This effect was due to fog resulting from the long exposure necessary. Then followed a zone in which some structure was fairly visible, and finally an almost black ring of shadow, due to the fact that the actinic value of the light illuminating the circumference of the field was almost *nil*. In no case had Mr. Osmond reproduced the fine ultimate structure of the dark areas of normal steels, and his photographs of pearlite conveyed a very poor idea of that constituent as it really existed in steels.

[1] "Journal of the Iron and Steel Institute," No. 1, 1890, p. 45.

Prof. Arnold. It was difficult to focus sharply at high powers, and isochromatic reproduction was practically impossible, so that constituents of quite different colour to the eye, yet of equal actinic value, appeared to be one substance on the photographic plate. He had several years ago made many attempts to reproduce the structure of steel photographically, but his results, like those of Mr. Osmond, were unsatisfactory and misleading. It was a significant fact that Mr. Osmond, when describing the more delicate structures of steel, could not appeal to his photographs, but had to construct, for the purpose of conveying his meaning, very diagramatic diagrams.

With reference to Prof. Roberts-Austen's attempt to confute his magnetic experiments and deductions, he had shown by experiment that while quenched high-carbon steel possessed the maximum capacity for permanent magnetism, manganese steel was absolutely non-magnetic. The magnetic identity of the two classes of steel was re-established by Prof. Roberts-Austen as follows:—(1) A quenched cemented bar in which the carbon varied between 1·7 per cent. and 0·35 per cent. of carbon, when scratched with a diamond, could not be marked in the zone containing 0·7 per cent., but was capable of being scratched in the high- and low-carbon zones; (2) A bar containing 1·5 per cent. of carbon was quenched between 1,000° C. and 1,100° C. in ice-cold water. A similar bar was also thus quenched from 800° C. The latter exhibited nearly three times the capacity for permanent magnetism possessed by the bar first named. Moreover, the bar quenched at 1,100° C. contained two distinct microscopical constituents. Therefore the magnetic properties of quenched high-carbon steel were directly analogous to those of manganese steel or an alloy of iron containing 22 per cent. of nickel. The cogency of this train of argument he emphatically disputed.

A very valuable point had been raised by Mr. Head concerning the micro-structure of No. 2 steel containing 0·4 per cent. of carbon. The diagrams showed that taking perfectly pure iron under favourable conditions a perfect set of geometrical crystals would be obtained. If a little carbon was added it did not in normal steels dissolve and diffuse itself through the iron, but it separated into little patches, as represented in No. 1, which were strictly local. Those brown patches he might call, without entering into the theory of the question, true steel. As the carbon increased more of the dark steel areas came in, until invariably (that was not a result of a single observation, but of many), when in iron 0·45 per cent. of carbon was reached there was a mixture in practically exact proportions of crystals

of iron and crystals of steel. At 0·9 per cent. of carbon the material was completely steel; so that 0·45 per cent. itself constituted a semi-critical point. If an engineer were using a material with more carbon than 0·45 per cent., that metal was virtually steel, and all the characteristics of steel would be, as it were, in the ascendant. On the other hand, below 0·45 per cent. of carbon, the iron, as in the actual diagram was the predominant partner, and there would be registered the characteristics more of iron than of steel. That was invariably found in a large series, and had been carefully confirmed by Mr. Stead and Mr. Andrews, with entirely independent samples. Prof. Arnold.

He desired to thank the President for his remarks with reference to the attitude of engineers towards researches of that kind. He (the President) had pointed out very properly that engineers to appreciate those researches wanted them applied as soon as possible to their daily work; and had mentioned a specific case of a steel rail, and certain minute cracks occurring in various kinds of steel. He might be allowed to mention that while the present research was upon the action of carbon and iron and the formation of carbides, another research, now almost ripe for publication, was upon the action of sulphur on iron and upon the effect of sulphides, and the results that had been obtained were altogether startling. He made the statement—he was almost prepared for some kind of incredulity, but he made it with all confidence, founded as it was upon careful experimental data—that sulphur would have in the near future to be very carefully watched by engineers. The quantities which were now considered satisfactory were under certain conditions absolutely fatal. It was necessary to remember when speaking of 0·05 per cent. of sulphur that it existed as a sulphide of iron, and that that sulphide of iron would probably be about 0·3 per cent. He had found cases in which sulphide had liquated between the crystals of steel in very thin attenuated lines, and had utterly destroyed the cohesion, so that in a fairly mild steel casting, he had proved that by cultivating, as it were, these meshes for experimental purposes, the strain would be only 15 tons per square inch. On annealing, they altered the form of the 0·30 per cent. of sulphide present from long lines, cutting the crystals into pieces—into small globules of sulphide—with the result that the stress on the casting increased from 15 tons to 27 tons. He had been lately investigating the case of a screw-shaft which broke in actual practice. In that case, the effect of the sulphur was clearly shown, and the distribution of the sulphide in the interior of the shaft, together

Prof. Arnold. with its structure, fully accounted for its sudden rupture. The problem in the future for iron-makers would be, in order to give engineers trusty and reliable material, to endeavour to get an iron absolutely free from sulphur. [The PRESIDENT hoped that Prof. Arnold would communicate the results of his further experiments to the Institution.] He considered the rail specification of Mr. Windsor Richards to be practically and theoretically sound, embodying the maximum hardness consistent with safety but still keeping the properties of the iron predominant. The amount of manganese mentioned undoubtedly tended to prevent the sulphur forming those fatal meshes of sulphide, maintaining the latter in its less dangerous globular form, as he would show in the Paper he hoped shortly to submit to the Institution in response to the encouraging invitation of the President. High silicon, on the other hand, undoubtedly tended to throw out meshed sulphide. In view of the rigid scrutiny to which Mr. Andrews had subjected Prof. Arnold's sections, and of the large number of sections of commercial steels Mr. Andrews had prepared to confirm or refute them, he was much gratified with the favourable decision to which he had come after nearly two years of patient work, during which he had examined almost every class of railway material in addition to large masses such as propeller shafts. He had listened with great interest to Mr. Stead's description of his careful and ingenious experiments. The micro-chemical identity in one bar of the structures described in the Paper was very striking, and the fact that the saturated zone only ranged from 0·85 per cent. to 0·92 per cent. of carbon seemed conclusive testimony that the theoretical saturation point had been correctly formulated at 0·884, the mean of Mr. Stead's figures being 0·885. He thanked Mr. Stead for his frank disagreement with the supposed enunciation that in hardened steels the areas of the sub-carbide and structurally free iron would correspond with the light and dark areas of normal steels. On reference to the Paper it would be seen that the statement that the dark areas were nearly proportional to the carbon had reference only to unhardened steels. However, as Mr. Stead's objection was repeated by other microscopists, it was evident that he had not rendered the matter clear. As Mr. Stead pointed out, the dark areas in the hardened sample of steel containing 0·38 per cent. of carbon were much larger than those in the normal steel, being indeed about 70 per cent. of the total areas instead of 42 per cent. The question of this diffusion of the dark areas of normal steel into the structurally free iron was a very complicated one. It had been shown by Mr. Osmond

that in hardened unsaturated steels the higher the temperature of quenching the greater the intrusion of the dark into the light areas. In considering the cause of this effect it appeared that two possible factors were involved: 1. The reduction of the sub-carbide to a still more attenuated carbide as suggested by Mr. Stead. 2. The mechanical diffusion of the sub-carbide through the plastic iron augmented by the internal disturbance produced by the shock of hardening. He considered the second explanation as the more probable of the two, because, before the change point at $A_R 1$ was reached, most of the sub-carbide had again massed itself into separate areas with the exception of the residue (several times referred to in the Paper as the cause of the pale-brown crystals of iron), the change point of which seemed to take place at $A_R 3$ before the change at $A_R 1$ had commenced. The solution theory failed to explain the undoubted microscopical fact that nearly the whole of the carbon of unsaturated steels was found in distinct areas before the thermal change marking the formation of normal carbide had commenced. Unless a sub-carbide existed, which, before the critical point $A_R 1$ was reached, gathered itself into masses distinct from the iron, there did not appear to be any reasonable explanation why the Fe_3C was not evenly diffused through the iron of normal unsaturated steels. Prof. Arnold.

He could hardly express the interest with which he had listened to Mr. Hadfield's description of his success in preparing the long desired alloy of practically carbonless iron and manganese. The matter came upon him as a surprise, and he congratulated Mr. Hadfield on his success, the more so because the speaker's attempts to produce such an alloy had always ended in failure. Of the great theoretical importance of Mr. Hadfield's results there could exist no doubt. The β-iron theory, as he had originally attacked it some years ago, assigned allotropic functions, that was α or β-iron producing powers, to all the elements of steel. Manganese was supposed to harden iron as carbon did, except that a larger quantity was required to render stable the hard modification. On the other hand, the two chief softening elements, that was to say, elements tending to produce α iron were gravely stated to be sulphur and phosphorus. Now, for the first time, the alleged β-iron producing power of manganese had been put to a direct experimental test with the result stated by Mr. Hadfield, whose further experiments on this matter would be watched by steel metallurgists with great interest. He welcomed Dr. Anderson's suggestion that iron and manganese formed a double carbide which accounted for the hardness of Mr. Hadfield's alloy, but

Prof. Arnold. Dr. Anderson seemed to have overlooked the fact that one of the strongest planks in the allotropist's platform was the allegation that the hardness and non-magnetic properties of manganese steel were due to β iron and not to a triple compound such as that suggested by Dr. Anderson, the existence of which had long been urged by Mr. Hadfield and himself.

In reply to Dr. Kennedy, he agreed that from an engineer's point of view the elastic limit was the most important mechanical feature, but his compression tests had been made solely to measure as nearly as possible the comparative fluidity of the various steels. The increasing diameter of the masses of the softer steels as the stress increased affected the accuracy of the results, but in the direction of minimizing rather than exaggerating his curves. With reference to Dr. Kennedy's request for another point in the curves at 1·1 per cent., the observations plotted contained points at 0·89 per cent., 1·2 per cent., and 1·47 per cent. Only the annealed curve seemed to claim in the smallest degree the point mentioned by Dr. Kennedy, and in this case the separation of graphite at 1·2 per cent. clearly accounted for the broken nature of the return curve. Surely in the hardened curve Dr. Kennedy did not hold that steels incompressible at 0·89 per cent. and at 1·2 per cent. would become compressible at 1·1 per cent. Possibly Dr. Kennedy had not noticed that there were three points on the zero line. He agreed with Mr. Wrightson as to the important part played by stress as one of the factors determining the mechanical properties of steel, but stress was very different from allotropic transformation.

Mr. Wrightson. Mr. WRIGHTSON observed, in reply to Mr. Stead, that the expansions of the 15-inch balls of grey- and white-iron were in each case measured by observing the amount the top half of the mould was lifted as the ball cooled. The measurements had been made by inserting a wedge-shaped gauge between horns cast on the top and bottom boxes of the mould, which, being protected by a considerable thickness of loam, were not affected by any heat from the metal. The top of the mould was only taken off after the expansion had ceased, when the measurements during contraction were taken by careful callipering of the horizontal diameter. The details of the experiments had been described in 1880.[1] He could not agree with Mr. Stead that a white ball should not expand more than a grey ball. This was based upon the erroneous assumption that the density of the white and grey iron "were

[1] Journal of the Iron and Steel Institute, 1880, p. 11.

the same when in a fluid state." It was well known that in Mr. Wrightson. iron-founding the allowance for contraction in moulding was about 50 per cent. more for white than for grey iron. This was confirmed by the experiment illustrated in *Fig. 2*, where, in the final contraction of the 15-inch grey and white balls, the former contracted $\frac{8}{100}$ inch in diameter, while the latter contracted about $\frac{13}{100}$ inch. Although relatively correct, these figures, as stated in the Paper, were not quantitatively so. Taking, therefore, the actual practice of iron-founders, the allowance for lineal contraction in grey foundry iron was $\frac{1}{8}$ inch in 15 inches, or 0·83 per cent. In white iron it was $\frac{1}{8}$ inch in 10 inches, or 1·25 per cent. These expressed in volume were approximately—

	Per cent.
For grey iron	2·50
" white "	3·75

If Mr. Stead's determination of the specific gravities of solid grey and white Cleveland iron be taken as 6·94 and 7·48 respectively,[1] the densities of each in its liquid state would be 2·5 per cent. and 3·75 per cent. respectively less than those solid densities. This gave the liquid density of grey iron as 6·77 and that of white iron as 7·2. This result, in the case of grey iron, nearly approximated to that given by using the instrument for measuring the increase of volume by buoyancy described in the Paper; while, therefore, it may be correct to say that white and grey iron were the same chemically in the molten state, they were quite different in the most important physical property of density, and, as shown in the Paper, they differed also in their range of expansion and contraction in passing from the liquid to the solid state. In reply to Mr. Head, the sphere was selected as the most suitable shape for experiments, taking into account the special object in view, which was not to make a sound casting but to observe what occurred after the mould was once filled with molten iron. The feeding of the "git," which was the ordinary method of preventing the formation of top cavities, would have counteracted the effects he wished to examine. With regard to Mr. Head's doubts as to the Mint experiments proving that a fall of temperature took place in compressing iron within the welding range of temperature, the researches of Carnot seventy years ago, and of Dr. James Thomson and Lord Kelvin forty-five years ago, conclusively proved that any substance that contracted in heating or expanded in cooling, must

[1] Journal of the Iron and Steel Institute, 1880, p. 45.

Mr. Wrightson. under pressure, cool and have its melting temperature lowered, unless it be assumed that perpetual motion, or the production of work from no expenditure of energy, was possible. His experiments in 1879 had shown that cast-iron possessed the property; and the experiments at the Mint proved that wrought-iron also possessed it. It was important for two reasons:—1st, If it could be demonstrated; then the welding property of iron would probably be identical with the property of regelation in ice, which also cooled and lowered its melting-point between certain critical limits of temperature when under pressure; 2nd, In experiments with the purest of mercantile iron, such a demonstration would show whether the phenomenon depended upon the presence of carbon, or was purely physical and inherent to the iron alone. The experiments appeared to show that iron, whether carbon was present or not, possessed this remarkable property, though the presence of carbon in quantities such as found in No. 4 foundry iron or high-carbon steels interfered with the collateral property of welding which was found only when the amount of carbon was largely reduced. With regard to Mr. Blount's remarks, he had experimented on bismuth, which possessed the property of expanding when cooling, the volume increasing 2·3 per cent. If two rods of pure bismuth were held in a Bunsen flame until melting commenced at the ends and then taken out of the flame and pressed together at intervals of a second or two, a time arrived during the cooling when a distinct adhesion was felt between the ends of the rods corresponding with the adhesion of two welding-surfaces.

He had, in conjunction with Prof. Roberts-Austen, made a number of experiments at the Mint with the instrument described in the Paper, upon bismuth, copper, lead, tin, zinc and silver, with the object of ascertaining the liquid densities of those metals.[1] The determination of the specific gravities by this method corresponded closely with the determination made by Mr. Robert Mallet by a different method, showing that the diagrams were reliable for this purpose. In the case of bismuth, considerable flotation occurred immediately the ball was immersed, and the diagram produced by the instrument gradually fell to the line of liquid volume. Tin was the only other of these metals which, after first sinking, showed in its diagram a slight rise above the line of liquid volume.

The changes of volume in certain metals when melted had been investigated by a totally different method by Messrs. F. Nies

[1] Proceedings of the Physical Society of London, vol. iv. p. 195, and vol. v. p. 97.

Mr. Wrightson

and A. Winkelmann,[1] who had found in the case of tin that the density of the molten liquid was only 0·7 per cent. greater than that of the hot solid at a temperature just below melting-point. They had arrived at the general result that not one of the metals examined would justify the general assertion that "bodies contract on becoming solid," but the experiments rather favoured the view that metals when solid, at a temperature close upon their melting-points, were less dense than when molten. It might be observed, however, that Messrs. Nies and Winkelmann's method gave very small differences in density between the liquid and the melting solid state in most of their experiments. The difficulty in observing adhesion in the case of bismuth with its 2·3 per cent. change of density, would imply a much greater difficulty in the case of tin with its 0·7 per cent. of change. It apparently required larger changes of volume, such as were found in ice and in iron before the property of regelation or welding could be rendered evident to the senses. As to the difference between melting together and welding, with an electric-welding machine, melting together of various metals was easily effected. In such cases the hammering would do no good but probably harm. Hammering and pressure had a function of their own when applied to iron or low-carbon steel within the critical range of temperature. His view was explained graphically in the appendix to the Paper.

Correspondence.

Prof. Behrens.

Professor H. BEHRENS, of Delft, had for some years been working in the field covered by Prof. Arnold, and was able to confirm a great part of the experimental results, and to wholly accord with the conclusions, set forth in his Paper. In a book,[2] published in 1894, he had advanced a similar theory for the hardening of iron-carbon alloys; and had explained the effect of annealing and of mechanical work upon metals. He had shown that reduction of grain was brought about by alternation of annealing and mechanical stresses. A clear insight into the nature and origin of Dr. Sorby's "pearly constituent" had been given in the first Paper, chemical and microscopical research being supplemented by mechanical tests and by investigation of the thermal and magnetic

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[1] Sitzungsberichte der Akademie der Wissenschaften zu München, 1881, part I, p. 63.

[2] "Mikroskop. Studien über Metalle und Legierungen." Leop. Voss, Hamburg & Leipzig.

Prof. Behrens. behaviour of the specimens. The combined results of these various methods of research gave overwhelming evidence in favour of what might be termed the theory of unstable alloys, advanced first by Dr. Sorby, and against that of allotropic modifications, advocated by Mr. Osmond and Professor Roberts-Austen. Results of microscopical examination of Siemens-Martin steel of various grades had been given[1] by Mr. Osmond, in the main facts agreeing with those at which Prof. Arnold had arrived. He thought that especial attention should be directed to the significance attached to the alloy containing 0·89 per cent. of carbon. Microscopical and physical research here agreed so closely with the selection made for practical uses, that there was full justification in marking 0·89 per cent. of carbon as the saturation point for carbon-steel. He desired to congratulate the Author upon this happy conception, as on the able and conscientious manner in which he had worked out a broad experimental foundation for further research on the nature of steel. With regard to the hard alloys of iron with silicon, chromium, etc., his results[2] concerning those with carbon, chromium and tungsten, might be found of interest.

Prof. Le Chatelier. Professor H. Le Chatelier, of Paris, considered the existence of the saturation point, corresponding to 0·89 per cent. of carbon in the steel, was established by the concordance of the microscopic, recalescence and magnetic observations described in the Paper. He regarded Prof. Arnold's experiments, however, as insufficiently extended to define with certainty the conditions of the formation of graphite in steels. As these were expressed they were in contradiction to the best established laws of physical chemistry.

Two of the results expressed in the Paper appeared contradictory. (1) The separation of graphite took place below 685°, at a temperature at which the carbide $Fe_{24}C$ could not exist; and (2) The separation of graphite was partial, and was not produced until the contents in carbon were greater than corresponded to the formula $Fe_{24}C$. That the separation of the graphite took place below a temperature of 685° C. was not shown by any of the experiments, for the annealing had been effected by heating to 1000° C. for seventy-two hours. It would be necessary to anneal at lower temperatures than 700° C. and to show the isolation of the graphite. Indirect reasons were meanwhile required

[1] Comptes Rendus, April, 1894.

[2] "Ueber krystallisirte harte Verbindungen in Cementstahl und in Legirungen des Eisens mit Chrom, Wolfram und Mangan." Zeitschrift für analytische Chemie, xxxiii.

Prof. Le Chatelier.

to confirm Prof. Arnold's statement. If the separation of graphite took place above 700° C. it would be found as much in tempered steels, provided that the heat before tempering had been sufficiently prolonged. It must therefore be admitted that graphite was formed below 700° C. according to the reaction $Fe_3C = 3\,Fe + C$; and it was impossible for the quantity of graphite isolated to be connected by a necessary relation with the total quantity of carbon existing in the steel. In all the phenomena of chemical equilibrium the bodies transforming themselves were separated and simply juxtaposed without dissolution or mixture in variable proportions, thus:—

$$CaO, CO_2 \rightleftarrows CaO + CO_2$$

$$S \text{ (octahedral)} \rightleftarrows S \text{ (prismatic)}$$

$$MgOSO_3, Na_2OSO_3 \rightleftarrows MgOSO_3 + Na_2OSO_3,$$

a condition also realized in the reaction,

$$Fe_3C \rightleftarrows 3\,Fe + C.$$

The heating of the system by constant pressure tended to bring about in it, at a certain temperature, an opposite state. This temperature of transformation was, for carbonate of lime, 812°, for sulphur 95·6°, and for astrakanite 25°. Below 812° it tended to exist only as the carbonate of lime, and above it as lime and carbonic acid. For the carbide Fe_3C in like manner there must exist a point of transformation, still unknown, above which the carbide and below which the mixture of iron and of graphite might be stable. A steel ought then, whatever its amount of carbon, to be exclusively constituted at the ordinary temperature of iron and of graphite without carbide. But all chemical transformations did not always result from such actions as super-saturation, superfusion, etc. Sometimes these were but temporary; the system in time returning to the normal state of equilibrium corresponding to the actual temperature. Prismatic sulphur at ordinary temperature, returned to the condition of octahedral sulphur. The system could otherwise remain indefinitely in its unstable state, at the ordinary temperature, in the cases of the yellow variety of oxide of lead, or massicot, and tempered steel. The normal decomposition of carbide of iron with iron and carbon, in the generality of cases, could not be brought about. Different circumstances, generally ill-defined, and governing which the laws were unknown, might favour this return towards the state of equilibrium; and it was possible that the excess of carbon contained in

Prof. Le Chatelier. the steel might be one of those circumstances. The normal state of equilibrium ought to be the same whether there be much or little carbon; but the ease of return might be greater in presence of an excess of carbon. Among the circumstances which favoured the return towards the equilibrium state might be mentioned the action of small quantities of foreign matter. Thus traces of benzin caused the immediate transformation of prismatic into octahedral sulphur, and small quantities of solutions of potash caused the transformation at 100° C. of massicot into litharge. Similar actions could be conceived to facilitate in the steel the decomposition of the carbide of iron. It was without doubt to a similar action that the formation of graphite in grey cast-iron and in malleable cast-iron was to be attributed, in presence of small quantities of silicon, aluminium and manganese; it was not impossible that hydrogen and nitrogen could play a similar part. In presence of one or other of these bodies all the carbon in the steel might be reduced to the state of graphite, and at the same time the temperature of transformation might be determined. It would be also possible to connect the dissociation of the oxide of carbon in the presence of iron, discovered by Sir Lowthian Bell, with that of carbide of iron. The following reaction would take place:—

$$2\,CO + 3\,Fe = Fe_3C + CO_2$$
$$Fe_3C = 3\,Fe + C$$

which, as a final result, was equivalent to

$$2\,CO + Fe = Fe + C + CO_2.$$

That would require the temperature of transformation of carbide of iron to be above that of dissociation of the oxide of carbon, or above 490° C.

Mr. Hibbard. Mr. HENRY D. HIBBARD thought that Professor Arnold's Paper marked a distinct advance in knowledge as to the influence of carbon on iron; but the multiplicity of theories regarding the subject bore witness to the difficulties surrounding it. None of these speculations as yet accounted for the various phenomena exhibited, although the carbo-allotropic theory advanced by Mr. H. M. Howe might possibly be made to do so when thoroughly developed.

It might be stated that the saturation point, given by Prof. Arnold as 0·89 per cent. carbon, had been known many years. He had found from hundreds of tests of open-hearth spring-steel, ranging between 0·7 per cent. and 1·5 per cent. of carbon, that specimens containing between 0·85 per cent. and 0·90 per cent.

carbon proved the strongest of any in the testing machine. The limited number of experiments reported in the Paper bearing on this point, corroborated the fact that with this amount of carbon a certain maximum was reached. The formula $Fe_{24}C$ represented steel with this amount of carbon sufficiently well. The substance of Prof. Arnold's theory seemed to be that in unhardened steel there existed the carbide Fe_3C diffused in about seven times its weight of metallic iron; while in hardened steel this carbide was combined with the seven parts of iron giving the subcarbide $Fe_{24}C$. Fe_3C contained 93·33 per cent. Fe and 6·67 per cent. C. $Fe_{24}C$ contained 99·12 per cent. Fe and 0·88 per cent. C. In unhardened steel carbon had the effect of raising the strength about 800 lbs. per square inch for each ten-thousandth part contained. At this rate, a carbon bar of the same weight as a given steel bar of 1 square inch cross section, would have a tensile strength of 8,000,000 lbs. Taking, however, carbon at the specific gravity of the diamond 3·55 and the specific gravity of steel at 7·85, it would give the carbon a strength of 3,620,000 lbs. per square inch. Pursuing the subject and observing the influence of carbon in hardened steel, it would be found from results given in Mr. H. M. Howe's Paper, lately read before the Iron and Steel Institute,[1] that 0·21 per cent. carbon raised the strength of hardened steel to about 225,000 lbs. per square inch, or 180,000 lbs. above that of wrought-iron. This showed an increase of strength in the steel of 8,570 lbs. per square inch for each ten-thousandth part of carbon. On the basis of the calculation previously referred to, carbon apparently had the strength in hardened steel of 38,750,000 lbs. per square inch. If now, finally, it were assumed that the hardening and strengthening effect was produced by the carbon lost in the colour carbon or Eggertz test, say one-half of the whole, there would result an effect from carbon in hardened steel of 77,500,000 lbs. per square inch. That was to say, considering the strength of the metallic mixture (hardened) of iron and carbon known as steel, and allowing to each ingredient an effect proportionate to its percentage, if the iron were given a tensile strength of 45,000 lbs. per square inch, there must be allowed to the carbon a strength of at least 38,750,000 lbs. per square inch. If smaller test-bars were used the influence of carbon would probably be found to be even greater, with larger bars it would be less. Similar examination of the effect of other elements in steel also led to imposing figures. It did not seem possible that the

Mr. Hibbard.

[1] Journal of the Iron and Steel Institute, 1895, p. 9.

Mr. Hibbard. mere substitution of any carbide for metallic iron or one carbide for another, could produce such considerable consequences. There must be a profound change in the constitution of the metal. The operation of hardening steel was assuredly accompanied by chemical change, but it was doubtful whether that change alone was sufficient to account for the great effects produced. The discovery of the subcarbide of iron $Fe_{24}C$ was apparently considered by Prof. Arnold to account for all the phenomena due to the influence of carbon on iron. It seemed that even this left the question still open, for the mere substitution of $Fe_{24}C$ for other arrangements of the same iron and carbon could not be held sufficient to account for the hardness and strength of quenched steel. From the Author's remarks, it might be inferred that he viewed with something like contempt any effort to explain the influence of carbon on iron by consideration of the molecules and their functions. So averse was he to looking beyond the crystals for information, that when he could not see them and gave no proof of their existence, he assumed them to be present. It still seemed that an explanation of the nature of the influence of carbon on iron must begin or end with the molecules. If one molecular arrangement gave crystals between which existed planes of weakness; and another arrangement, caused, it might be, by quenching or cold working, prevented or replaced the crystalline structure, doing away with such planes of weakness, that of itself should work important changes in the physical properties of the body.

Dr. Hopkinson. Dr. John Hopkinson confined his remarks to the magnetic qualities of iron. He suggested, with regard to Prof. Arnold's magnetic experiments, that it would be useful if the resistance of the magnetising coil were given, or the current instead of the potential; it would then be possible to calculate the magnetising force. The rods were not long enough to give satisfactory quantitative results of magnetic property. Experiments of this kind were better made either with long wires or closed rings. The magnetic properties of many varieties of steel and also of iron had long been known with considerable accuracy;[1] a knowledge of these properties showed that though they were affected by the presence of carbon, other substances also had a great effect; for example, tungsten and chromium greatly increased the coercive force, that property on which permanent magnetism depended, without any change in the quantity of carbon. In like manner manganese destroyed the magnetic

[1] Phil. Trans., 1885 (Part ii.), p. 455.

properties altogether, that was, it reduced the permeability from many hundreds to about 1·5, and this without material variation in the carbon present. Dr. Hopkinson.

The magnetic properties of iron in relation to temperature were also well understood.[1] In the case of the purest iron obtainable, the permeability for small forces was continuously increased by heating to a certain temperature, 860° C. in the case of one sample; at this temperature it reached the value of over 10,000, but with a further heating of two or three degrees only the magnetic property entirely disappeared. The suddenness of the change was startling. The magnetic properties of iron were no mere laboratory properties; upon them depended the production of thousands of horse-power from dynamo-machines.

On the chemical questions concerning carbon in steel he offered no opinion, but he would call attention to the fact that in the case of the purest iron there was at 860° C. a physical change taking place suddenly as great as that from solid ice to liquid water. The properties of an alloy of nickel and iron containing 25 per cent. nickel had been referred to by Prof. Roberts-Austen, and were most remarkable.[2] Nickel steel, as received from the maker, was at all ordinary temperatures and at higher temperatures non-magnetisable. On being cooled to − 50° C. it became highly magnetisable and remained so when it was heated to the ordinary temperature of the air. When heated it preserved its magnetic quality until a temperature of over 600° C. was reached, when a sudden change occurred. It returned to the non-magnetisable state, and on being cooled remained in that state until it had been cooled again to a temperature of − 50° C. This change of magnetic properties was accompanied by other changes. The resistance in the non-magnetisable state was about 30 per cent. greater than in the magnetisable condition. The breaking stress in the non-magnetisable condition was perhaps 50 tons per square inch; in the magnetisable condition it was 80 tons, and, most remarkable of all, the density was completely changed, being, in the non-magnetisable condition, about 8·15, and in the magnetisable condition 7·97. These changes could surely not be associated in any special degree with the presence or absence of carbon, they depended upon the peculiar alloy of iron and nickel. At all events

[1] Phil. Trans., vol. 180 (1889), A. p. 443; Journal of the Institution of Electrical Engineers, vol. xix. p. 10; Proceedings of the Royal Society, vol. lii. p. 228.

[2] Proceedings of the Royal Society, vol. xlvii. pp. 23 and 138; vol. l. p. 121; vol. xlviii. p. 1.

Dr. Hopkinson. there remained the fact that between − 50° C. and 600° C. this particular material could exist in two different states very different from each other, and it could be changed from one to the other either on the one hand by cooling it to − 50° C., or on the other by heating it to 600° C.

Mr. Howe. Mr. H. M. Howe, of Boston, U.S.A., had not understood, when he had discussed [1] them several years ago, that novelty attached to the theories (1) that above the critical temperature the carbon of steel spontaneously formed a definite sub-carbide,[2] the excess of iron or of carbon, as the case might be, separating as free iron (ferrite), or as normal carbide (cementite, Fe_3C); (2) that sudden cooling preserved this condition, so that hardened steel consisted of, and owed its properties to, this constitution, and especially to the sub-carbide; (3) that in slow cooling the excess of free iron or of normal carbide persisted, while the sub-carbide split into a mixture of free iron and normal carbide, which under favourable conditions interstratified, forming the pearly constituent (pearlite). The novel point in Prof. Arnold's theory which demanded attention was that the sub-carbide had the definite composition $Fe_{24}C$. That, as the carbon-content progressively increased, the rate of variation of certain properties, whether of hardened or of very slowly cooled steel, or of steel in its normal state, passed a critical point, or, at least, that the corresponding curves were not perfectly regular, had long been known. The exact position at which Prof. Arnold had fixed these critical points, and in his inference from it, were, however, new. The microscopic evidence brought in the Paper alone was direct in its bearing on the composition of the sub-carbide; that of the position of the critical points was at best only corroborative or suggestive. And of the micrographs, those of hardened steel alone told directly of the constitution of this substance, for the constitution of slowly cooled steel could at most make suggestions as to the constitution from which it had been derived during slow cooling, *i.e.*, that which existed above the critical temperature, and which sudden cooling preserved in hardened steel. Prof. Arnold's direct evidence thus consisted of three micrographs of hardened steel, two of which were very poor. Even these did not seem in accord with his theory, for, according to it, that of the steel containing 1·47 per cent. of carbon should, by his own calculation (Table IX),

[1] "The Metallurgy of Steel," vol. 1, by Henry Marion Howe, New York, 1890.

[2] Though he had not used the word "sub-carbide," he had pointed out that the carbide of hardened steel contained much less carbon than normal carbide, and this was all that the name "sub-carbide" here meant.

contain 11 per cent. of normal carbide, yet the only foreign substance visible here formed a slender network, which seemed to occupy much less than 11 per cent. of the whole. Mr. Howe.

The structure of hardened steel had still not been revealed under any conditions by Prof. Arnold, nor even such imperfect micrographs as he had obtained except by quenching "from a nearly white heat." The structure of steel had, however, been admirably disclosed by Mr. Osmond,[1] not only when hardened from a white heat, but when hardened from the lower temperatures which seemed to baffle Prof. Arnold completely. In view of Mr. Osmond's success, and of the clearness with which Mr. Sauveur's preparations had revealed the structure of steel which he (Mr. Howe) had hardened from various temperatures, Prof. Arnold's imperfect success was surprising. In a series of some forty-five admirable photo-micrographs of steels of three different compositions, and hardened under systematically varying conditions, Mr. Osmond had shown that the characteristic component of hardened steel was a substance which might be regarded as a distinct mineral species of definite properties, and which was presumably a sub-carbide of iron. He had named it martensite. Assuming that quenching preserved the structure which existed at the moment of immersion, it was found that each of the two steels, those of 0·14 per cent. and 45 per cent. of carbon respectively, of which Mr. Osmond gave enough micrographs to enable the matter to be followed, consisted, when above its critical range of temperature, of two separate components, grains of free iron and crystals of martensite.[2] As the temperature rose beyond the critical point, the quantity of martensite progressively increased, while that of ferrite decreased correspondingly, the martensite progressively absorbing the iron of the ferrite. As the quantity of carbon in such a steel was the same at these different temperatures, and as the quantity of martensite thus varied so widely, he saw no escape from the inference that the composition of the martensite varied as widely, and that the conclusion that the characteristic component of hardened steel had the definite composition $Fe_{24}C$, and, indeed, that it had any definite composition, was erroneous.

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[1] Méthode Générale pour l'Analyse des Aciers au Carbone, Bulletin de la Société d'Encouragement pour l'Industrie Nationale, May, 1895. This constitutes so important an advance in metallography that one who has not mastered it is very seriously handicapped in discussing the present questions.

[2] For clearness he here disregarded the two transition species, sorbite and troostite.

Mr. Howe. That the pearly constituent did not uniformly contain 0·89 per cent. of carbon was indicated by its volume in some of the micrographs (Plate 1). Thus annealed steel No. 6 had about the same volume of pearly constituent in the micrograph as annealed No. 2; yet by analysis it had only one-tenth the amount of normal carbide (to which the volume of pearly constituent in these two should be proportional) as the latter, which indicated a wide difference in the composition of this substance. So normal steel No. 6, though by analysis it had only about half as much normal carbide as normal steel No. 4, yet from anything shown by the micrograph, had by no means proportionately less of the pearly constituent, or of its parent material.

Further evidence that the pearly constituent did not uniformly contain 0·89 per cent. of carbon was furnished by Dr. Sorby, who had found apparently about 90 per cent. of it in steel containing apparently some 0·49 per cent. of carbon, indicating that here the pearly constituent contained some 0·54 per cent. of carbon; and by Mr. Osmond, who had found that it constituted nearly the whole of slowly cooled steel of 1·24 per cent. of carbon, which carbon-content should therefore be here approximately that of the pearly constituent, and that in one and the same steel its volume varied greatly with the temperature from which slow cooling occurred, implying corresponding variation in composition.

The phenomenon which formed Prof. Arnold's second (thermal) line of evidence, that, as the carbon-content rose above 0·89 per cent., the spontaneous retardations of heating and cooling, instead of becoming correspondingly more marked, if anything diminished in intensity, was certainly a most important and remarkable one, which his own records seemed to corroborate. He had long ago noticed that with 1·10 per cent., and even with 2·41 per cent. of carbon, the retardation seemed no more intense, if indeed as intense, as with 0·88 per cent. of carbon. Perhaps as the carbon-content rose beyond 0·89 per cent. the retardations changed in their distribution in such a way as to mask their increasing quantity, as, for instance, by spreading out over a longer range, so that doubt arose as to what the normal curve was, and thus as to what delay was represented by the retardations.[1] The

[1] Thus Mr. Osmond had found that even at 35° above the upper end of the a_1, and 60° above its crest, the normal carbide had not changed fully to sub-carbide, or at least its carbon had not yet diffused. This seemed to support his conjecture, as did the progressive encroachment of the sub-carbide (martensite) on the free iron (ferrite) as the temperature rose beyond a_1, and of the free iron on the sub-carbide as the temperature again descended towards a_1, an encroachment which in some cases covered at least 330° C., and probably much more.

calculation was not easy, and care must be taken to make all the conditions constant, because the heat evolved or absorbed had to retard the change of temperature of the furnace as well as that of the steel. If, however, the increase in the quantity of heat represented by the retardations really was not proportional to the increase of carbon-content above 0·89 per cent., the phenomenon was hard to interpret. At first sight it certainly seemed to support Prof. Arnold's theory, though of course only circumstantially and indirectly. On reflection, however, it was seen that the support was equivocal, for the phenomenon might be due to any one of a variety of causes. Mr. Howe.

He did not find the third (compressive) line of evidence, that 0·89 per cent. of carbon was "a point in the compression-curve of hardened steel at which molecular flow absolutely ceases," supported by the data. Passing by the fact that, both with 20 tons and 25 tons per square inch pressure, there was a point which, under other circumstances, Prof. Arnold would call critical, not at 0·89 per cent. but at 0·59 per cent. of carbon; it was found that, under these particular conditions of hardening, when the carbon-content reached 0·59 per cent. the compressive elastic limit almost equalled the compressive strength, so that but trifling flow was possible, apparently about 0·01 inch in a 1·126 inch test-piece. Further, that, between this carbon-content of 0·59 per cent. and one of 0·89 per cent., the possible flow became not only less than 0·01 inch but less than "practical." In the second footnote on p. 139, the language implied that with 0·89 per cent. there was some flow, but so little that it was not "practical"; this did not agree with his later assertion that here the flow had ceased absolutely. There was no evidence to teach even where the flow, which was very trifling at 0·59 per cent. of carbon, became less than "practical"; it might have been anywhere between 0·59 per cent. and 0·89 per cent. of carbon, and, judging by the data given, probably much nearer to the former than to the latter. The "absolute" cessation of flow appeared, judging from that footnote, to occur beyond 0·89 per cent. of carbon. He here found no support for Prof. Arnold's theory.

As to the fourth (magnetic) line of evidence, he had plotted the data given in the Paper and found nearly as sharp flexures in the curve at 0·59 per cent. and 0·74 per cent. of carbon as at 0·89 per cent. If the results for 0·74 per cent. of carbon were changed so as to force the curve to fit the theory, all that the data now meant was that somewhere between 0·75 per cent. and 1·20 per cent. of carbon the curve changed its direction.

Mr. Howe. The change might occur at 0·89 per cent. of carbon, but this was not sufficiently proved; indeed, the curve suggested 0·75 per cent. of carbon rather than 0·89 per cent. On p. 140 it was asserted that throughout the mechanical tests there was a tendency of most of the curves to break at the point 0·89 per cent. of carbon. The only mechanical tests of hardened steel which he found were the compression tests, the curve of which broke not at 0·89 per cent. but at 0·59 per cent. of carbon. Breaks in the curves of unhardened steel would be only moderately suggestive as to the constitution of hardened steel, for reasons which he had already pointed out. Examining the curves and plotting those which were not supplied, among the normal steels he found that in only one out of the five, viz., the compression curve, did the sharpest bend lie at 0·89 per cent. of carbon, and that curve was to be suspected, because intrinsically improbable, and indeed in part discredited by Prof. Arnold. In none of the others was there the slightest indication of any break or maximum point at 0·89 per cent. of carbon, but in three of them 0·89 per cent. of carbon actually lay at the smoothest part of the curve. Of the five curves for annealed steel, those for tenacity and elastic limit were smooth, with only slightly more deflection at 0·89 per cent. of carbon than at 1·20 per cent. Those for elongation, contraction of area, and compression, indeed showed somewhat sharp bends at 0·89 per cent. of carbon, but this bend was in each case due to the curve's teaching that, as the carbon-content rose from 0·89 per cent. to 1·20 per cent. and 1·47 per cent., the metal became progressively more ductile and yielded more under compression. This was so improbable that he gravely suspected these curves. Indeed, curves founded on such extremely scanty data carried little weight. Their maxima rested on the properties of only two pieces of steel. Out of eleven curves five had their sharpest bend elsewhere than at 0·89 per cent. of carbon, two had about the same there as at 1·20 per cent. of carbon, and the remaining four, which alone had their sharpest bend at 0·89 per cent. of carbon, were all doubtful. But even if the last four lines of evidence were all that was claimed for them, they would be only suggestive or corroborative. For while 0·89 per cent. of carbon would naturally be a critical point if Prof. Arnold's theory were true, it might equally well be one if that theory were untrue. He saw no reason why the properties, mechanical, thermal, and magnetic, of a sub-carbide of varying composition might not pass critical points as that composition varied.

Prof. Arnold's disproof of the allotropy of iron seemed to be :— (1) Carbon cannot exist at high temperatures in "mere solution," because, as the carbon-content increases, the volume of the carburetted or pearly constituent increases with it; and (2) unless the carbon so exists at high temperatures in mere solution there can be no allotropy. Mr. Howe.

He believed that some steps in this reasoning must have been omitted. He failed to see that the correspondence between the volume of the pearly constituent and the quantity of carbon present, which had long been known, had any bearing on the question whether the carbon was combined or dissolved at temperatures above that at which that pearly constituent was formed. Admitting that the carbon at those high temperatures was combined, he could not see that it disproved allotropy even in the sub-carbide itself. Mr. Osmond's micrographs indicated that, at temperatures above the critical point, the low-carbon steel contained not only the sub-carbide (martensite), but also in many cases free iron (ferrite). It could not be advanced that the existence of chemical union between the iron and carbon of the sub-carbide in any way disproved the allotropy of the free iron of the ferrite.

With regard to allotropy, three questions should be considered :— (1) whether iron at high temperatures assumed a different allotropic state; (2) if that state could be preserved by sudden cooling; and (3) if preserved, what part it played in the hardening of steel. The evolution of heat when carbonless iron cooled past the critical range of temperature (Pionchon and Roberts-Austen), and the loss of the magnetic properties in that range (Gilbert, Pionchon and Roberts-Austen), seemed to show that carbonless iron underwent some chemical or physical change in that range, in which change, because of the apparent absence of all other bodies, the iron alone was concerned. Such a change he regarded as by definition allotropic. He failed to see how any definition of the word could be framed to exclude this change in iron without excluding other changes to which it was applied. The fact that quenching might nearly triple the strength of iron which did not contain enough carbon (only 0·06 per cent.) to account readily for this change[1] indicated that this allotropic condition of iron might be preserved by sudden cooling.

[1] Le Chatelier, Journal of the Iron and Steel Institute, 1894, I, p. 210. He (Mr. Howe) had obtained similar results with steel of 0·09 per cent. of carbon. Transactions of the American Institute Mining Engineers, XXIII, 1893, p. 531.

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Mr. Howe. Recent experiments of his own pointed in the same direction. It was known that steel was hardened if quenched from a high enough red heat, but not if quenched from a slightly lower one. Here, then, was a critical range in which the power of being hardened was lost and in it the condition of carbon changed. In cooling through this range low-carbon steels underwent more than one retardation. His experiments tend to show that part of the loss of the hardening power preceded the change in the condition of carbon, and thus that it was connected with some change other than that of carbon condition. By elimination allotropy remained as the most probable cause; but as the determination was most difficult, he did not regard his results as final.

It seemed most probable that hardening was two-fold in its nature. Above the critical temperature the low-carbon and the medium-carbon steels in general appeared to consist of a mechanical mixture of free iron, apparently in the β or γ state, and of sub-carbide. Their hardening appeared to act in part by preserving the allotropic state of the free iron, and in part by preserving the sub-carbide. The higher-carbon steels, when well above the critical temperature, in general appeared to consist almost solely of sub-carbide, and their hardening probably acted chiefly by retaining this sub-carbide undecomposed. Whether the sub-carbide be of α iron or of β or γ iron was difficult to show. The statement that β iron was supposed to be stable in hardened steel was liable to mislead. β iron was supposed to be retained by the quick cooling, but, like all allotropic substances so retained, it must be unstable. The action of carbon in retarding the allotropic change was probably a case of catalysis, so common a phenomenon that Prof. Arnold must be familiar with it. Perhaps, in treating of the annealing of castings and of forgings, he had confounded two processes, which, though alike in name, had important differences in nature—two different species of one class. Indeed, the annealing which Prof. Arnold used differed so much from the normal annealing either of forgings or castings as to form still another species. Reasoning from one species to another should be close and cautious. He questioned the assertions as to the influence of lead and glass on each other, as to the relative importance of the present results, as to the relative accuracy of the micrographs and those attainable by direct photography, as to the influence of the several different conditions of the normal carbide on the physical properties, and the definition of "body." The corroboration of the theory from practical observations seemed to be without force.

Mr. Jenkins.

Mr. HENRY C. JENKINS observed that it was stated in the first Paper, "If it can be proved that at high temperatures the carbon still remains in combination with the iron, the foundation of the β-iron theory will be destroyed and its superstructure must naturally collapse," but that it was recognised that the carbon in steel at high temperatures should, according to the β-iron theory, be in a free state. As it was possible, indeed of frequent occurrence, for changes of a chemical character to be incomplete, it was necessary to prove that carbon was not in a free state in the iron under those conditions in order to controvert the α β theory. Evidence in the Paper, however, showed the proof was impossible, for the presence, now confirmed by Prof. Arnold, of free tangible graphite, liberated from the steel under experiment, had to be accounted for. Even could it be proved that some carbon remained combined with the iron at high temperatures, the foundations of the α β iron theory would not be destroyed; a modification would be pointed out of its details which had only been stated provisionally. It was admitted in the Paper that iron changed its state, and until evidence was forthcoming that that change and the hardening of steel were independent phenomena, it was rational to connect them as cause and effect.

It was generally accepted that, in fully annealed steel, carbon existed mainly as a free carbide Fe_3C, the presence of which was indicated by microscopical examination by Dr. Sorby, while it was isolated and examined by Sir Frederick Abel, Dr. Müller, and by others; but whether in hardened steel the carbide existed in a dissolved state in the rest of the iron, or whether it was decomposed by the heating so that free carbon remained dissolved during hardening, or whether other carbides were formed, had not been conclusively shown. The existence of $Fe_{24}C$ and also of $Fe_{10}C$ was believed by Prof. Arnold, who, however, offered no evidence that did not tend with equal force to show that hardened steel was a solid solution of Fe_3C in an excess of iron, or with still greater force that the hardness of steel was due to the association of the carbon with a modified form of the iron. So far from undermining the α β theory, Prof. Arnold, as already mentioned, admitted one of its most prominent facts; and in a series of calculations actually used a constant quantity of heat which could only be assigned to a change in the state of iron itself. Allotropy only would justify such a proceeding.[1] This constant quantity was also the amount evolved by the purest iron that had ever been prepared, iron so pure that no known source of error was presented.

[1] Journal of the Iron and Steel Institute, 1894, part i. p. 189.

Mr. Jenkins. The assumption was made in the Paper that the β condition would necessarily be one of condensation. This might be so, but it was not probable; and, as the passage of α iron into the β condition required heat energy (the constant quantity to which he had referred), it was but reasonable to conclude that β iron had more intrinsic energy per unit mass, a condition that generally held good with bodies that had already exhibited the phenomena of dissociation. The masses of the α and β molecules might be the same, and the β molecules have greater atomic movement, but it is more probable that the latter were smaller. It did not appear to be recognised by Prof. Arnold, that the molecules of a dissolved body could exert pressure on the solvent, but it was well known that they did so.[1]

The chemical evidence adduced in the Paper as to the existence of the new carbides was unsatisfactory. It was not doubted that, in the normal and slowly cooled specimens the carbide Fe_3C separated out; the difficulty was with the hardened specimens. It could not be said that Prof. Arnold obtained any Fe_3C, but rather "residues" containing varying amounts of carbon and iron, and large amounts of what was assumed to be water, to the extent of 46 per cent. in one case, and of 28 per cent. in another. Oxygen might have been present as oxide of iron, and if this were so it would materially alter the character of the evidence, disregarding the admitted presence of other impurities. The brown and black powders left in these instances by Weyl's method were in all probability mixtures of (1) a small quantity of Fe_3C, which owed its fine state of division to the unfavourable conditions of quick cooling under which it was formed, and (2) large variable amounts of carbon liberated in a sufficiently coarse state of division to escape gasification. If the assumption that the hydrocarbons evolved during solution were from the decomposition of some new carbide to which the hardness of the steel might be due, it was difficult to see how, as he had stated in a recent Paper[2] to be the case, the amount of gas evolved was the same from the hardened and annealed steels. Again, if the carbon of the hardened steel had been liberated from combination it would have been in a state of great molecular activity, and instead of getting the large amount of "hydrate" carbon (a name yet to be justified), the whole should have been lost as gas.

It could not be admitted that the mechanical tests threw much

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[1] Ostwald, *passim*.

[2] Proceedings of the Chemical Society, vol. lxv., 1894, p. 798.

light on the points at issue, though Prof. Arnold made no attempt Mr. Jenkins. to elude the fact that free carbon could and did exist in steel. He questioned the statement, that the best mechanical test for molecular rigidity was that of compressive stress, which introduced frictional resistances at the surfaces of the dies and complex internal stresses in the specimen; and was therefore one of the worst tests of molecular rigidity that could be applied. The well-known test by torsional vibrations was, however, not referred to.

The points of discontinuity, where they existed, in the curves, merely indicated that 0·9 per cent. of carbon was about the amount which, under ordinary conditions, was capable of existing in a completely dissolved state in steel. The phenomena of saturation and of supersaturation must not be confounded with that of combination.

The conclusions of the Paper traversed much that was established as being of fundamental importance in chemical physics. It was generally accepted that dissociation occurred with rise of temperature, and was accompanied by absorption of heat-energy. There were very few apparent exceptions to the rule, and these doubtless would fall into line as they were better understood. But it was assumed, without direct experimental data, that what seemed to be a great chemical law did not apply to the case of iron, and he viewed the free carbon to be due to dissociation of Fe_3C at a low red heat, and that the latter body had itself been formed by decomposition of the hypothetical $Fe_{24}C$ at a higher temperature. Not only was this in contradiction to all thermal evidence and chemical analogy, but even the existence of the $Fe_{24}C$ appeared for the present to rest on his own further assumption, that at high temperatures steel with 0·9 per cent. of carbon must be a definite carbide. It was more in accordance with probability to consider the carbon at these high temperatures to be in some state analogous to that of solution, particularly as white cast-iron would hold four times at much carbon as was required for Prof. Arnold's assumption. And white cast-iron, which was very hard, behaved as steel with its properties too much exaggerated to be useful, as though all that caused brittleness and hardness in steel existed there to a greater degree. On such faulty reasoning it would seem that white cast-iron with 3 per cent. or more of carbon was the compound rather than 0·9 per cent. of carbon steel, and in quite an arbitrary way Fe_7C might be chosen which would have 2·97 per cent. of carbon. All this carbon was held without more than a trace of graphite being seen—if only the iron be quickly cooled.

Mr. Jenkins. The highly viscous, almost rigid condition of the iron at a red heat, would do much to prevent segregation of the carbon when the degree of concentration of that element was below a certain value; and this would account for the absence of the graphite from the samples of steel with less than 0·9 per cent. of carbon. The light and dark zones in the micro-sections, upon which Prof. Arnold built so much, were in all probability due to the changes in the mass starting from a large number of centres at one time, the iron being both a viscous solvent and a good conductor of heat.

As to the thermal observations it was difficult to comprehend Prof. Arnold's meaning, and he had not given the actual curves from which his deductions had been made. The division of a time by a rate did not, for instance, give a length. His experiments were open to serious objections quantitatively; it had been long since pointed out by Dr. Hopkinson that the method of record adopted was only suitable for qualitative purposes, the use to which Mr. Osmond had originally put it. The final curve, *Fig. 25*, was not, however, a simple linear function of the carbon contents, but a more complex curve, the special interest of which lay in the fact that, to obtain it, it was actually assumed that there was a change (A_R3), constant in amount for all samples of steel, and independent of the carbon changes. The only constant body present beyond the small amount of impurity, which could possibly be a cause of sufficient magnitude to account for A_R3 was the iron, and change in that, independently of the carbon, was allotropy; the iron had not been fused.

But the sub-carbide theory was not the only one that would point to a maximum evolution of heat at a saturation point, the α β theory offered a means of also predicting this, and that without difficulty. According to this latter theory, the carbon held, for a time, a fractional portion of the iron in the β form, the fraction increasing with the amount of carbon present, as well as with the rapidity of the change of temperature. The residue of the iron was free to combine and form Fe_3C as soon as it could overcome the affinity between the β iron and carbon. Until the carbon contents reached a certain value all the excess of α iron could be satisfied, Fe_3C being formed with evolution of heat, and probably more α iron; but when that amount of carbon was exceeded, the α iron, which was decreasing owing to the increase of the carbon, was in too small a quantity to attract and satisfy the latter body. The heat evolution which had been hitherto increasing with the carbon at once showed a decrease in amount, owing to the formation of the carbide Fe_3C in smaller quantity. The point of maximum

evolution of heat would enable an opinion to be formed as to the amount of α as compared with that of β iron when certain proportions of carbon were present, and under given rates of cooling, which latter condition would obviously alter the position of the maximum itself. The tendency of α iron to combine with carbon and form Fe_3C assisted the completeness of the allotropic change by causing the removal of both the carbon and the product of the change from the sphere of action; and the presence of a second metal, such as chromium or nickel, that would take up the excess of carbon, and leave the α iron free, would indirectly favour the retention of the β form by the remainder of the iron. Mr. Jenkins.

In view of the fact that Prof. Arnold had not succeeded in obtaining more than 85 per cent. of the carbon present in his mild steel No. 2 as normal carbide under the most favourable conditions, the existence of $Fe_{24}C$ could scarcely be based on the assumption that, at high temperatures and with relatively far less iron present, an excess of Fe_3C existed and remained stable. If it did exist there, he did not see why it was not obtained when the No. 6 steel was chilled, for it was elsewhere in the Paper advanced that the conditions of chilled metal approximate to those of the same when heated. Instead of obtaining $\frac{0 \cdot 05}{0 \cdot 89}$, as in the case of the No. 4 steel, Prof. Arnold considered that he obtained $\frac{0 \cdot 06}{1 \cdot 47}$; and on his own premises there would be no free normal carbide in the former case, and more than 8 per cent. of the whole sample as free Fe_3C in the latter. He asked why graphite and not Fe_3C was obtained in the richer steels. The assumption that this graphite was formed as a decomposition product at low temperatures, in the absence of any pressure, was not in accordance with analogy and remained to be justified; and it must be noted that a better relative yield of Fe_3C was reported from No. 4 steel than from either the No. 2 or the No. 6 sample.

The hypothetical body $Fe_{24}C$ was considered to have a "remarkable capacity for permanent magnetism." This might be the case, but the well-established non-magnetic properties of steels were not accounted for, such as that of the No. 4 sample, at temperatures higher than Ar 3, and where, according to Prof. Arnold's theories, so much of this magnetic $Fe_{24}C$ existed. It was, moreover, well known that the purest varieties of iron had the highest permeability, and that the resistance to magnetic change of state of steel increased with its carbon contents; and although negative evidence was adduced from the behaviour of man-

Mr. Jenkins. ganese steel, the positive evidence of the nickel variety was overlooked. It was still easier to account for the known behaviour of iron and steel by the β theory that iron is an allotropic body than by that advanced in the Paper; and notwithstanding so much experimental work, he had not produced either $Fe_{24}C$ or $Fe_{10}C$, nor, it must be submitted, any positive evidence of their existence.

Mr. Kreuzpointner. Mr. P. KREUZPOINTNER, of the Pennsylvania Railroad Company, considered Prof. Arnold's Paper a valuable addition to metallurgical knowledge. The results, as presented in Tables III, IV, V, seemed to show that, what were called the normal conditions might, after all, be unstable. Steels Nos. 1½, 2 and 3 were those used mostly by engineers. It was assumed by Prof. Arnold that the steel he used remained permanent in its behaviour within the limits of safety and calculations. This, however, he could hardly expect of a metal the elasticity and strength of which were affected by heat to the extent shown in Tables III and IV. It would indicate that steel of the chemical composition given would require a large factor of safety, if it were subjected to frequent heating and cooling, in addition to the strains the metal might have to sustain. Provided that frequent heating, though at lower temperatures than those given, had a tendency to change the nature of steel, producing fatigue, similar to the effects of repeated strains continued slightly above the elastic limit, it was necessary to know to what extent the elastic limit could be relied upon in an emergency. For this purpose it ought to be a tolerably stable quantity, within the limits of calculated stresses and the influences tending to weaken the structure or parts of the structure. Experience and investigations seemed to show these influences to be more deteriorating if the strains were variable and thermal changes took place at the same time. In such cases it might be preferable to anneal the metal before using it, while for structures subjected to simple strains, only without thermal influences more than those of natural surroundings a cold rolled or hard hammered steel might be preferable.

The practical uniformity of elongation of the normal and annealed steels, Nos. 1½ and 2, was surprising in view of the accepted theory that annealing tended to produce greater ductility. The micro-structure of these steels pointed to a change of carbon, while the large drop in elastic limit would indicate a softening of the individual crystals sufficiently to prevent elastic reaction. This seemed a case of the allotropic theory of iron, producing a loosening or relaxation of the molecular cohesion of the individual

particles, impairing their elasticity, while the form of the iron remained in a hardened state in both cases, preventing the expected stretch taking place. A similar irregular behaviour was observed in the three high-carbon steels, Nos. 4, 5 and 6. It would have been interesting to have had a comparative test with the same steels as received from the rolls and when hardened. Mr. Kreuzpointner.

A key-note was sounded by Prof. Arnold when he said, "If the cohesive force acting between the facets of crystals is from any chemical, thermal or mechanical cause seriously weakened, the metal will appear very brittle, owing to rupture under sudden shock occurring along the weak juncture lines, in spite of the fact that the molecular cohesion may be perfect and the individual crystals ductile, etc." He had little doubt of this apparent double nature of steel when in a certain condition, being responsible for a good deal of the distrust with which steel has been regarded for a long time. Many of the formerly so-called mysterious breaks of structural parts of steel were probably due to the same cause, namely, the condition of steel as described in the Paper. The fact was often overlooked that in steel there was a conglomerate of irregular shaped, various-sided particles, each of which was a unit of its own, but all of which are joined together by their sides into a more or less homogeneous mass by cohesion.

Whatever the capabilities might be of the individual particle or crystal to resist destructive forces, if the force holding the particles together by their sides or facets was inferior to the force or cohesion holding the molecules together which made up the particle or crystal, then the strength of the metal to resist extraneous forces, which tended to destroy the metal, would be the difference between the molecular cohesive force and the intercrystalline cohesive force. The metal was strongest if both forces were maximum and equal. The metal was weakest if the crystals were hard, but were held together so loosely that a slight blow or shock would scatter them in all directions. Between these two extremes all variations and combinations of strength might be found. It was, no doubt, often within the power of the steelmaker to avoid this double nature of steel which was prejudicial alike to the interests of the producer and consumer.

The nick and straight line, as shown in the diagrams of Charpy's researches, which had been advanced as an argument in favour of allotropy, seemed to denote a quality of practical value to the engineer. If that stage in steel, indicated by a nick and straight line, without increase of strength, was produced in obedience to a law, it would be of importance to know whether

Mr. Kreuzpointner. its appearance could be hindered, promoted or regulated at will by heat or mechanical work, or both. He had frequently observed this nick and straight line during testing, without, however, being able to follow up the history of the material exhibiting the phenomena, except in so far as it seemed to appear more often and most distinctly in material finished at a high initial heat. The phenomenon could be observed as well in the tensile as in the torsion machine, and without the aid of automatic registering apparatus when making tensile tests. A punch-mark was made in the section of a highly-finished test-piece well away from the fillet, and after the specimen was gripped and a light load applied for "set" a fine scribe-line was made on the section by taking 8 inches, the length of test-section, on a pair of finely-pointed dividers, steadying one point in the punch-mark and scribing a fine line on the section with the other point. Load is then applied in increments of 500 lbs. or 1,000 lbs. and released and the weight run back to zero frequently. After each release of the load the point of the dividers was set into the original scribe-line to determine if permanent set had taken place; in fact the dividers were never removed from the punch-mark during the operation. The elastic limit had been reached when, on the last release, the point of the dividers did not coincide any more with the original scribe-line, but remained outside on its edge, making the original line double the former thickness if a new line were now drawn. After the elastic limit had been reached a continuous load was applied in increments of 1,000 lbs., and a line drawn at each load without stopping the machine until the maximum load was reached. At between 3,000 lbs. and 6,000 lbs. after the elastic limit a stretch took place without increase of load covering a distance between the last and the following line of from $\frac{1}{16}$ inch to $\frac{1}{4}$ inch. After this "drop" the lines scribed followed each other closely and regularly again, widening in distance gradually towards the maximum load if the metal was uniform. If it was not uniform there would be irregularity of the distances between the lines, but even then the "drop," or interval of stretch, corresponding to Charpy's nick and straight line, would stand out prominently if it appeared at all. It was interesting to watch thus the pulsation of the metal under strain.

The impression which the observation of the phenomenon conveyed was, that, if the elements composing the steel were in what might be called a simple or primitive state, the effects of the work which the metal was performing during test would result in a uniform and gradual extension, other things being equal, under

Mr. Kreuzpointner.

a uniformly increasing load; conceiving the increasing load on the test-piece to result in a crowding together of the crystals to the degree as the load increased. This crowding together of the crystals into a more compact mass would explain the increase of strength up to the point where the external force became greater than the internal cohesion; there would likewise be reason to expect that as the crystals were forced upon one another and elongated, this elongation would be as uniform as the increase of load and subsequent compacting of the metal. If any irregularities in stretch took place they might rather be expected toward the period of maximum load where the breaking-up and disjointing of the groups of crystals was greatest. Nor would it be quite reasonable to assume the presence of hollows and open spaces between the crystals which were closed under the first few thousand pounds of load after passing the elastic limit. This interval of stretch as observed in a tensile test, or nick and straight line as given on the diagram-sheet of a torsion test, seemed to be more pronounced in soft steel and steel finished at a high initial heat, though the writer has observed again greater intervals of stretch in harder axle steel of 80,000 lbs. per square inch than in softer boiler steel of 60,000 lbs. per square inch, which fact, by the way, would alone refute the assumption of open spaces between crystals.

Judging from the phenomenon observed and here briefly described, it would seem as if the elements composing the steel were not in such a simple or primitive state. He had mentioned these observations because the question was of interest to the engineer when and how this phenomenon in steel might affect the stability and safety of a structure, since cases might arise, and unquestionably did arise, where the calculated load exceeded the elastic limit. If this nick and straight line, or interval of stretch period, indicated a change from the crystalline structure of steel to an amorphous structure, he believed, though he was unable to offer evidence for the support of his belief, that it was a favourable property of steel for the engineer; because this stretch between loads acted as a cushion, as it were, interposing itself between the destructive force and the cohesive force when a structure or part of a structure happened to be overstrained. The amorphous structure displacing the crystalline structure during the nick-and-straight-line period, the chances favoured a prevention of sudden fracture through the adhering faces of the crystals; hence a new, and perhaps more favourable condition of the steel to counteract the effects of the overstrain.

Mr. Mc-William. Mr. A. McWilliam, of Wednesbury, considered that all the evidence, especially the quantitative determination of the heat evolved, its amount being almost exactly proportional to that of the sub-carbide present and changed to carbide, the sharp return of the curve at the 0·89 steel, and the uniform occurrence of the critical points at that percentage of carbon shown by the other methods of examination, undoubtedly pointed to the existence of a chemical compound of iron and carbon of formula $Fe_{24}C$; and also to this carbide as the chief cause of the hardness of quickly-cooled steels perhaps assisted by the intense hardness of the Fe_3C in supersaturated hardened steels. The results achieved went to prove the efficiency for the purposes of research of the method so strongly advocated and so ably carried out by Prof. Arnold—that of correlation by every suitable means on the same cast of metal. He believed that steel which would harden by cooling from a high temperature in air or by any other method of cooling which was as quick or quicker, owed its hardness, not to the 10 per cent. to 15 per cent. of impurities present, but to the action of these elements in holding the carbon as carbide under "normal" conditions; as, after the prolonged heating in hæmatite of a piece of rectangular section, it was found on cooling in the usual way for hardening that the corners were soft. This he mentioned as the result of one experiment which, however, he intended to repeat in a way which would show the property more clearly, although the experiment was sufficient at the time for the end in view. In endeavouring to apply the β-iron and atomic volume theories to practice and to the explanation of phenomena observed in the steel works, he was constantly being met by the difficulty that this was a case in which the action was not apparent; and the real facts, owing to other causes, would be at variance with those which these theories would indicate. The remarks of their strongest advocates in 1894 were, therefore, reassuring, namely, that the impurities in the steels made by Prof. Arnold would entirely change the action. These steels being the purest recorded, he had laid the matter aside to await the results of further research. But he had noticed with surprise that while 0·2 per cent. of impurities was considered sufficient to determine the rejection of other conclusions, the deductions from Mr. Howe's experiments on a steel containing 1·5 per cent. of impurities, including 0·3 per cent. of silicon and 1·2 per cent. of manganese, were accepted as sufficient to claim him as a convert to β iron. The attempt of Mr. Howe to explain the hardness of steel by assuming a carbide of β iron seemed

untenable under the definitions found in classical chemical treatises, for since allotropic modifications of elements contained only the atoms of that element arranged differently, compounds made from different allotropic modifications of an element were bodies, the molecules of which contained an atom or atoms of that element in chemical union with an atom or atoms of another element or elements. Even assuming that a certain grouping were a β iron and that the chemical union of carbon with this were of such a nature that the molecule of β iron joined with an atom or atoms of carbon, this would simply be a carbide of iron; and although there might be allotropic forms of carbides of iron, no one of these could be correctly called a carbide of β iron. The hardness of carbide of iron did not even disturb the elementary conceptions of the fundamental facts connected with chemical compounds that the properties of a compound generally differed much from the properties of its constituents. The solution of practical problems seemed, therefore, to lie in the study of ultimate chemical composition, mechanical tests, microscopic and thermal examination, and the determination of the way in which the carbon was distributed and combined. The present research threw considerable light on many points, and with further experiment on similar lines should still further elucidate such matters as the following:—the fact that in a series of crucible-steel bars the coarsest fracture usually indicated the toughest material, while in other metals it was very often the reverse; the cohesion of the powders of even crystalline metals under great pressure, as in the experiments of Professor Spring. Of the utmost importance for future work was the modifying influence of the other elements usually present, on the effects of carbon, also the nature of the changes produced by hot and cold work and the effect of annealing on ordinary commercial steel castings. Mr. McWilliam.

With reference to the composition of the saturated steel, he had some years ago occasion to determine amounts of carbon for steels best suited for various kinds of work, and differing from the ordinary commercial steels in being exceptionally free from manganese and other impurities. As the result of experiments, he had fixed on the steel containing about 0·9 per cent. C. as the most satisfactory for making tools to stand hammering with a sledge on exceptionally hard work, the ordinary percentage used, with the usual quantities of other elements present, being somewhat less than this amount. A most important point clearly brought out was that the best of steels and the most "fibrous" of irons must be looked upon as crystalline bodies, and that the

Mr. Mc-William. molecular constitution also usually showed itself in the mechanical properties of these bodies by its effect on that crystalline structure. Although the effect of carbon on iron was made clear, there remained still a great amount of work to be done on similar lines with the benefit of the new light thrown on the subject by the Paper.

Mr. Metcalf. Mr. WM. METCALF, of Pittsburgh, U.S.A., remarked that Prof. Arnold's conclusions were generally in accordance with experience, and considered the Paper a distinct advance in the field which it covered. To the steel-worker the mode of annealing was objectionable; for, as had been already pointed out, long continued exposure to high temperature caused the separation of the constitnents of steel, the carbon separating and segregating into the graphitic form to such a degree as to vitiate the good effect of hammering or rolling. The steel was rendered granular, not thoroughly ductile, and difficult to refine and strengthen by the hardening process. It seemed probable that if the annealing had been continued but one hour instead of seventy-two hours, different results would have been obtained; and it was a matter of surprise that the high steels Nos. 4, 5 and 6, had not been more affected than was shown by the results. The "saturation point," 0·89 per cent. or 0·90 per cent. of carbon, was especially interesting. He had called attention to this point about twenty years ago,[1] and it was well known to steel-makers, that for the largest variety of useful purposes, steel having that amount of carbon was the best. The confirmation of the saturation point by the delicate and accurate tests of Prof. Arnold should go far to establish it, and so to furnish another stepping-stone for farther advances. The existence of the sub-carbide, $Fe_{24}C$, could not be proven except by deduction, any more than the β iron of Mr. Osmond. It was hard to understand how a few degrees of difference of temperature would reduce a part of this sub-carbide and so reduce hardness in the whole mass, successively, degree by degree, until the softness and ductility of properly annealed steel was reached. If heat continued to be regarded as a mode of motion, in the whirl of molecules caused by high temperature, the carbon might be so thoroughly distributed as to be truly dissolved in the mass; the sudden arresting of motion would then prevent separation, and the carbon would remain in solution. If hardness were attributed to high tension in any hard crystalline body it was readily conceivable that every

[1] Transactions of the American Society of Civil Engineers, vol. v. p. 323; and Transactions of the American Institute of Mining Engineers, vol. v. p. 355.

degree of rise of temperature would increase motion or mobility, and reduce the hardness or rigidity. The calling of this solution a sub-carbide, $Fe_{24}C$, did not seem to furnish as clear an explanation of the hardening and softening phenomena, as the simpler theory of solution and tension. Mr. Metcalf.

Prof. FREDERICK C. J. MÜLLER, of Brandenburg, considered the conclusions deduced in Prof. Arnold's Paper were most valuable, and would form the basis for a more accurate theory of steel. He could not adopt the expression $Fe_{24}C$, as such complicated molecules would not exist at high temperatures; the simple Fe_3C dissociating at 800° C.; he preferred "0·9 C alloy." A difficulty in the sub-carbide theory was that the hardening influence in mineralogical sense of carbon was not a maximum near 0·9 C per cent. of carbon. In hardened steel containing more than 0·9 C per cent. of carbon Prof. Arnold ascribed a certain percentage of Fe_3C, and stated that the surplus Fe_3C augments the hardness of the sub-carbide with its own intense hardness. His observations of eight years ago,[1] which had been corroborated by other investigators, showed that the hardness of Fe_3C does not exceed the sixth degree of the Mohs scale, Felspar. The isolated Fe_3C powder would scarcely attack a glass plate, whilst a good file with 1·2 per cent. of carbon scratched quartz. Prof. Müller.

Mr. OSMOND recognised the value of the experimental data which Prof. Arnold's new and interesting Paper had given. But as regards the interpretation of these data, whether by theory, argument, deduction or induction, the divergence between them remained on all points absolute. Mr. Osmond.

Chemical.—Hydrate of carbon had been formed by Prof. Arnold with water from the residues left by hardened steels, and by steel No. 6 which was annealed; he had not formed it with the residues of steels numbered 2 and 4, either in their normal or annealed state. He seemed to think that the water sometimes combined with carbon and at other times did not. Why was this difference? It was because in these latter cases iron and carbon were nearly associated in the proportions required by the formula Fe_3C, and, moreover, if any part of carbon were taken for combinations with water, Fe_3C would no longer be found; the accuracy of the formula was therefore admitted *a priori*, and whilst several reasons pointed to the probability of the existence of the compound Fe_3C, it was not found by experiment in the normal steels. It was a question therefore, in the case of the normal steel No. 6 of

[1] "Stahl und Eisen," May, 1888, p. 292.

Mr. Osmond. free carbon which was not graphite. What was this new form of carbon? By what method of calculation or analysis had its existence been determined? Its formation was attributed by Prof. Arnold to the setting up of secondary couples between the crystallised carbide and the same carbide in the lamellar form; but why did not the lamellæ between themselves constitute these same couples and give rise to galvanic action? The loss of carbon was sometimes attributed to the decomposition of a supposed carbide, $Fe_{24}C$, in hardened steels, and sometimes to the decomposition of another supposed carbide, $Fe_{10}C$, in steels which had not been hardened, a purely arbitrary distinction. In fact these experiments made by the aid of Weyl's method, experiments which had been copied by Prof. Arnold from those made by Messrs. Werth and Osmond, published in 1885,[1] did not appear to add much to the information given by the old results. Apart from the known distinction between "carbide carbon" and hardening carbon (and each of these forms were liable moreover to give rise to further subdivisions), the interpretation of numerical results was singularly delicate; it pointed to the belief that the lamellar carbide of Dr. Sorby was not the same as the carbide which occurs in thick plates.

Mechanical.—The fact that the tensile strength of normal steels did not increase continuously with the percentage of carbon was far from being new. In 1878, Mr. Marché[2] showed that, in round numbers, the maximum strength, for steel containing 1·20 per cent. of carbon, was 100 kilograms per square millimetre (63·5 tons per square inch). A very similar maximum (61·65) had been found by Prof. Arnold, also for 1·20 per cent. carbon steel and not for that of 0·89 per cent., without, however, a corresponding minimum of extensibility. The compression tests also indicated a minimum of fluidity in steel which contained 1·20 per cent. of carbon and not 0·89.[3] Annealed steels could not be brought in line in the present discussion because the two samples which contained more than 0·89 of carbon also contained, the one 0·28 and the other 1·14 of graphite.

The curve of compressibility of hardened steels showed that the shortening under a charge of 100 tons per square inch ought to become nil when the steel contained about 0·62 per cent. of carbon and not 0·89. Beyond that point the method of testing did not reveal any distinction.

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[1] "Annales des Mines," 8th series, vol. viii. p. 19.

[2] Apud Deshayes: "Classement et emploi des aciers." Paris, Dunod.

[3] The minimum is distinctly marked under the charge of 20 tons.

To sum up, but little relation could be perceived between the results found by Prof. Arnold's experiments and those indicated by him as demanded by his theory. Mr. Osmond.

Microscopical.—The preparations and drawings of Prof. Arnold were very well made; and, excepting that he lacked a method for dissecting, as it were, the hard parts of hardened steels, his observations, where comparison is possible, generally confirmed his (Mr. Osmond's) own, but the apparent agreement between the theory and the figures was not sustained by a more complete knowledge of the subject. Undoubtedly, with a certain percentage of carbon, normal or annealed steels contained neither ferrite or cementite in independent masses as distinguished from pearlite; the early researches of Dr. Sorby had already shown this; but that definite percentage of carbon was not mathematically 0·89 as the existence of a definite carbide $Fe_{24}C$ would demand. The proof of this was to be found in the fact that in a bar of iron carburized by cementation from a single surface, a bar, therefore, in which the percentage of carbon varied in a progressive and regular manner, the region of the pure pearlite was not restricted to a simple line; in reality it occupied a band of a certain width and, according to the general laws of "diminution," one could allow a range of 0·20 per cent. of carbon for the interval between the appearance of the free ferrite and that of the free cementite. The hardened steels furnished still more conclusive objections. The capital error of Prof. Arnold lay in the belief that in every case the grains of pearlite in normal or annealed steels, and the grains of the supposed carbide $Fe_{24}C$, in the hardened steels, occupied identical volumes. The truth was, that in the non-saturated steels, the carburized grains constantly varied in volume with the temperature, until the diffusion of the carbon was complete in the whole mass,[1] so that recognition of the solubility of the carbon in red-hot iron could not be avoided, without admission of the solubility of the supposed carbide. In Figs. 21 and 23, Plate 4, the respective proportions of ferrite or of isolated cementite were manifestly smaller than the theory demanded. Drawings of the microsections of hardened steels Nos. 1, 3 and 5 were not given, but he could repair this omission by appealing to the results of his own micrographic work as embodied in the Paper to which he had just referred. The three drawings which were wanting in the series of hardened steels were those which would have proved the theory to be

[1] Bull. de la Soc. d'Encouragement pour l'Industrie Nationale, May 1895.

Mr. Osmond. untenable. Steel No. 5 (1·20 of C) ought to contain, according to the Table on p. 161, 5 per cent. of normal carbide (crystallized). It probably contained no normal carbide (crystallized). Steel No. 3 (0·59 of C) ought to contain, according to the Table on p. 161, 35 per cent. of ferrite. It really contained no ferrite. Steel No. 1 (0·08 of C) ought to contain 8 per cent. to 9 per cent. of the supposed $Fe_{24}C$. It really contained no $Fe_{24}C$.

Physical.—It was known that a maximum of recalescence corresponded to a definite amount of carbon, the exact percentage being undetermined. This maximum was placed at 0·89 C, or, to be more exact, between 0·74 and 1·20 per cent. The process, which attempts to express in calories retardations or pauses in the rate of the cooling of steel, was somewhat arbitrary.[1] It would be preferable to have the curves themselves in order to study the progress of cooling or of heating. The existence, however, of a maximum recalescence in the region under consideration, did not appear doubtful. Whilst prolonging the curves indefinitely on the hypothesis of the solution of the carbon, two considerations had been omitted; one was that dissociations were limited, and the other was that solutions became saturated.

Magnetic.—The criticisms of Prof. Arnold as to the magnetic properties were particularly unfortunate, and recoiled heavily upon himself. ***Rides? Mutato nomine, de te Fabula narratur.***

PROF. ARNOLD'S OBJECTION TO THE ALLOTROPISTS.

Mr. Osmond's contention, that because carbon steel at a full red-heat is non-magnetic, and cold manganese steel is also non-magnetic, therefore both owe their impermeability to the presence of β iron, has always seemed an argument devoid of cogency. It is indeed generally admitted that the molecular condition of suddenly-quenched steel approximates to that of the fully red-hot metal; and hence it is obvious that if both quenched carbon steel and manganese steel consist chiefly of β iron, which is non-magnetic, therefore quenched carbon steel is non-magnetic, which is absurd.

OBJECTION OF THE ALLOTROPISTS TO PROF. ARNOLD.

Prof. Arnold's contention, that the magnetic properties of steel afford strong evidence of the accuracy of his views, seems to the writer an argument devoid of cogency. It is indeed generally admitted that the molecular condition of suddenly-quenched steel approximates to that of the fully red-hot metal; and hence it is obvious that if both quenched carbon-steel, and the same heated at a full red-heat, consist chiefly of the same carbide $Fe_{24}C$, as this carbide at a full red-heat is non-magnetic, the quenched carbon-steel is non-magnetic, which is absurd.

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[1] The systematic discrepancy between the curves of heating and of cooling was the proof of this.

Answer of the Allotropists.	Possible Answer of Prof. Arnold.
We have never said that hardened steel consists of pure β iron, but of an alloy of β iron and α iron. The theory of the hardening is so little in opposition to magnetic phenomena that it accounts for, and unites, them.[1]	The supposed carbide $Fe_{24}C$ is an allotropic body.

Mr. Osmond.

It remained now to offer another explanation of the facts on which Prof. Arnold had thought it possible to base his system. In reality these facts could be reduced to a single one, of which the others were but the consequences, that is to say, the saturation of steel by carbon.

Let C^s be called the percentage in carbon, which produced saturation[2] in pure steel at the temperature of recalescence. Immediately above $a\,1$ during the heating, the carbide Fe_3C (or at least its non-magnetic isomer), possessed a certain pressure of dissociation which increased with the temperature. On the other hand, the dissolved carbon possessed a certain pressure which varied with the proportion dissolved, and also with the temperature; C^s was the percentage of carbon at which these two variables were equal at about 700° C. If this percentage were smaller than or equal to C^s, the dissociation of carbide continued without hindrance. If it were greater than C^s, the dissociation stopped. Now if the temperature was raised, the carbon thus set free would dissolve or deposit itself in the form of graphite, according to whether the pressure of dissociation was, at a given moment, smaller or greater than the pressure of the carbon dissolved to saturation. These explanations had not been devised for the purposes of this discussion; they would be found in the "Théorie Cellulaire." Certainly they were not complete; general phenomena had in this case a tendency to complicate themselves, and were liable to numerous perturbations. But though written *ten years* ago, they none the less accounted for the new facts which had been observed. This was especially true in the case of the fact, irreconcilable with the ideas of Prof. Arnold, that the proportion of Fe_3C which was isolated in hardened steels varied with the temperature at which the hardening had been effected.

It was not probable, of course, that Prof. Arnold would express himself as being satisfied; he had, moreover, yet another resource;

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[1] Philosophical Magazine, vol. xxix. 1890, p. 511.

[2] The experiments of the Author showed that C^s nearly corresponded with 0·90 to $\pm$ 0·10 per cent., approximately, in all pure steels.

Mr. Osmond. it was to subject his theory, which was absolutely impossible in its present form, to an allotropic transformation.

Prof. Ripper. Prof. W. RIPPER had had the pleasure of following the research described in the Paper by Prof. Arnold from its beginning, and could testify to the extreme care with which it had been carried out. The information given might be divided into two kinds, the first, that of great practical value to the engineer; and the second that dealing with the theory of the ultimate structure of the material. It was especially to the first that he wished to refer. The research had been carried out on 50-lb. ingots of the material, 3 inches square, so that the results were comparable with, and immediately applicable to, ordinary steel-works practice. The research dealt also with the actual influence of carbon on iron, and not of carbon in addition to various other elements. For this purpose the iron used was of great purity (Table 1), and the results therefore were of exceptional value.

The tensile strength of steel increased with the carbon up to 1·2 per cent. only; beyond which point it decreased, as shown in *Fig. 1*, p. 133. The tensile strength of rolled-steel bars, annealed, fell below that of the unannealed samples, considerably in the case of the high-carbon steels. The ductility of the rolled material was not much improved by annealing, but rather the reverse for high-carbon steels, as shown in *Fig. 2*, p. 133. For the rolled and forged bar, annealing was therefore not only unnecessary but undesirable. This, of course, did not hold for steel castings, in the case of which, for special reasons, annealing is of extreme importance.

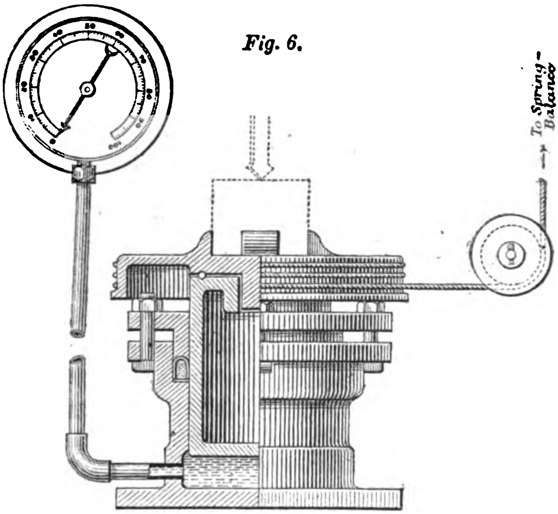
Microscopic observation had hitherto not proved a fruitful method of investigation, chiefly owing to the difficulty of obtaining concordant results. This was no doubt due to defective methods of preparation of the samples. A feature of the micro-sections, when properly prepared, was the absolute constancy of their general appearance, each steel repeating the identical characteristics of its class and condition with unfailing precision.

From all points of view the Paper was valuable to the metallurgist and engineer. It was now possible to examine at leisure the crystalline structure of the material, to observe its intercrystalline spaces, and to learn the actual effects upon the structure, of the various component constituents. That the changes occurring in steel might be explained by simple chemical processes the Author had well and clearly shown. The theory of a hard allotropic modification of iron containing carbon in solution had always appeared a tentative theory, more or less satisfactory until a more probable explanation was forthcoming. The β iron theory was

now no longer tenable in view of the new light thrown upon the structure of metals by the microscope. Prof. Ripper.

He had carried out a series of tests for abrasion hardness with the apparatus shown in *Fig. 6.* In a cylinder containing water, and through a watertight U-shaped gland of leather, worked a ram, on the top of which was a disk capable of rotating freely on ball bearings. The material was fastened to the disk as shown in *Fig. 6*, and the whole apparatus stood on the table of a drilling-machine. The abrasion hardness of the material drilled, or of the drill itself might be tested. Samples of drill-steel,

Fig. 6.



1 inch in diameter, had been tested for abrasion hardness. They contained 0·6, 0·9, 1·2, and 1·5 per cent. of carbon respectively. The pressure-gauge showed the vertical pressure per square inch of the drill upon the work, and the spring-balance registered the force necessary to balance the twisting action of the drill. The work done (W) was measured by the equation:—

$$W = 2 \pi r N \times P$$

where r was the effective radius of the disk to centre of cord; N, the number of revolutions of the drill; and P, the mean pull recorded by the spring balance. The readings of the pressure-gauge were useful for many purposes, but had been neglected in estimating the total work done, as that part of it in a vertical direction was only about 0·2 per cent. of the whole. If the

Prof. Ripper. work done be set off horizontally, and the depth of the hole drilled vertically, a curve might be drawn showing the relative abrasion hardness of the material of the drill. A soft drill would cause considerable friction in the hole as the feed was applied, and would show a high reading on the spring-balance, but the depth of hole drilled would be small. A drill of good material, with a hard

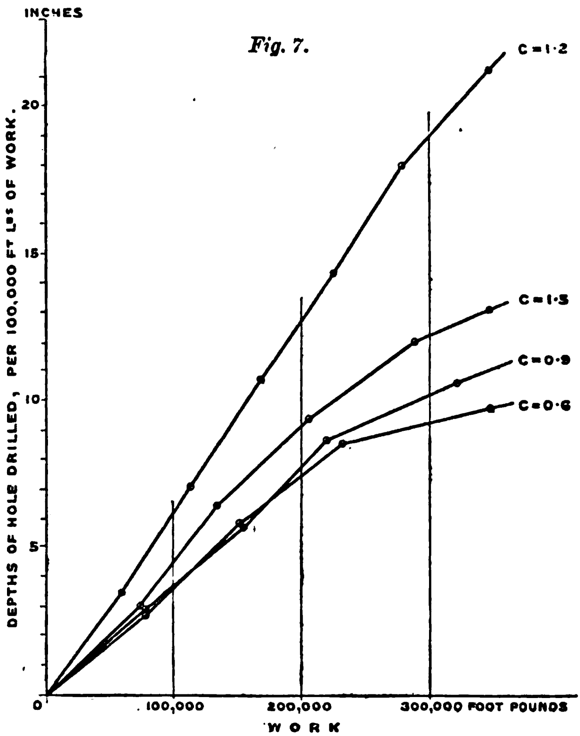


Fig. 7.

edge would, for the same readings on the spring-balance and revolution counter, drill through a much greater depth of material. These effects were shown in *Fig. 7*, from which it would be seen that a drill failed either from its being too soft or too hard. The drills containing 0·6 per cent. and 0·9 per cent. of carbon were too soft. That containing 1·5 per cent. of carbon was too hard, while the drill containing 1·2 per cent. had given the best results.

Mr. Sandberg. Mr. C. P. Sandberg considered that few engineers would now

be satisfied with the composition of rail steel favoured by Mr. Windsor Richards[1] with 0·35 per cent. to 0·40 per cent. of carbon, 0·10 per cent. of silicon, and 0·75 per cent. to 1·00 per cent. of manganese, and phosphorus not exceeding 0·10 per cent., owing to its softness. Steel with 0·50 per cent. of carbon was now mostly used for rails, even for hard climates. He thought engineers in America had brought about this extravagance in hardness, but there the compensation for loss of life through railway accidents was only £250 per person for all alike; for rather than use a heavier rail to meet the increased crushing-power of the heavier engines, the hardness of the section in use was simply increased. The tup-test was sacrificed, and the bending test was relied upon for safety. In America, percentages of 0·55 and 0·75 carbon had been reached for heavier sections, for instance, on the New York Central and Pennsylvania Railway. In England, on the contrary, railway companies chiefly relied upon the tup-test and not on chemical analyses, taking a limited deflection to represent the required degree of hardness. It was, however, noteworthy, that recently, some of the largest English railway companies had resorted to combine mechanical and chemical testing, such as the London and North Western, and Great Western Railways; a system that he had long adopted and worked to with every satisfaction to both makers and users, because these tests had been both carried out at the works during the rolling, so as to avoid any rejection of large quantities from the results of after testing. The Continental reporter at the International Congress also recommended rail-inspection by combined mechanical and chemical tests, both for safety and economy. As to carbon-estimation of steel by the coloration-test of Prof. Eggertz, of whom he had been a pupil at the School of Mines in Falun (Sweden), he had introduced the same in England through the late Dr. John Percy,[2] Hon. M. Inst. C.E. Few methods have proved of greater value to the metallurgy of steel, for granting that the combustion method was the more accurate of the two, the coloration-test was near enough for practical purposes; and every cast, even in large steel works, could be tested in an hour or two by this inexpensive and quick method. In the discussion of a Paper he had read before the Mechanical Engineers Institution at Sheffield in July, 1890, on "Rail Steel considered Chemically and Mechanically," Prof. Arnold had dealt with the influence of silicon, of which he had maintained that a certain proportion, say

Mr. Sandberg.

[1] Institution of Mechanical Engineers, Proceedings, 1890, pp. 331–333.
[2] See *Chemical News*, 30 May, 1863.

Mr. Sandberg. 0·1 per cent. was necessary for making the steel solid and free from blisters, which might afterwards turn out as thin scales, even with very hard steel. On the other hand, too high proportions of silicon, such as 0·2 per cent. and 0·3 per cent. caused pipy ingots, when they were of the present great weight of 1 ton or more. It was, however, noteworthy that such a large percentage of silicon could be used with safety and advantage in smaller ingots, such as quarter-ton ingots. Therefore, he considered the regularity of silicon as important as that of carbon in rail steel, a reasonable variation of both naturally being necessary for the maker, and of no practical harm to the consumer.

Mr. Sauveur. Mr. ALBERT SAUVEUR, of South Chicago, Illinois, expressed his admiration for Prof. Arnold's masterly work. Although he did not believe that he gave the true explanation of the hardening of steel, he considered the Paper a decided advance towards the solution of that vexatious problem. He would confine his remarks chiefly to the microscopical evidences, for these constituted the foundations of the theory, and because he had devoted much time, during nearly five years, to the study of the microstructure of iron and steel. It was shown most satisfactorily in the Paper how the microstructure of steel was related to its carbon content, and how it was affected by annealing. The influence which that change of microstructure, produced by annealing, must exert upon the physical properties of the metal was plainly shown. As to the cause of the hardening of steel, he believed that the evidence offered to prove the existence of a sub-carbide of iron of the formula $Fe_{24}C$ and the theory of hardening based upon it, was far from being conclusive. More convincing evidence was necessary to establish the existence of a binary compound of such extraordinary chemical structure. It had been reasoned that in unhardened steel a constituent was found, structurally made up of two components, which there were good reasons for regarding respectively as pure iron (carbonless iron), and a carbide of iron answering to the formula Fe_3C. This constituent had been called the "diffused normal carbide," or the "pearly constituent," but as there was no need to discriminate between the two distinct structures it was capable of assuming, he should call it the "composite constituent," a name which implied its structural character. Low-carbon steels, besides a certain amount of the composite constituent, contained much structurally free iron.[1] As the carbon increased the amount of structurally free iron decreased, until,

[1] He called structurally free iron the iron which, in the structure, existed by itself, in opposition to that iron which was present only as a part, as a structural element, of the composite constituent. And similarly for the carbide Fe_3C.

Mr. Sauveur.

when the steel contained 0·89 per cent. of carbon, it was entirely composed of the composite constituent. With new additions of carbon, structurally free carbide began to appear, increasing in quantity proportionally to the carbon. From the presence of that composite constituent, in unhardened steel, it was inferred, as Dr. Sorby had already done tentatively, that at a high temperature a definite compound of iron and carbon must exist, which, on cooling, splits into soft iron and the hard carbide Fe_3C. On this assumption rested the whole structure of Prof. Arnold's theory of the hardening of steel. Of the existence of such a compound he gave no direct evidence; it was an assumption the only *raison d'être* of which was that it accounted satisfactorily for the presence of the composite constituent of slowly-cooled steel. Then from the fact that a steel containing 0·89 per cent. of carbon consisted entirely of the composite constituent, it was concluded that, at a high temperature, the whole of the iron of that particular steel must be combined with the whole of the carbon—99·1 parts of iron to 0·89 part of carbon. In other words, that a carbide of the formula $Fe_{24}C$ existed at a high temperature. This, Prof. Arnold advanced, was an extremely hard compound, retained by sudden cooling, and producing the hardness of quenched steel. He had proceeded to show that his theory agreed well with the chemical, mechanical, physical, and magnetic behaviour of the steels. Much stress was laid upon the fact that at 0·89 per cent. of carbon the steels showed a critical point in the various tests to which they had been submitted, and it was considered a strong argument in support of the theory. At about 0·89 per cent. carbon the whole steel was made up of the composite constituent, while softer steel contained structurally free iron, and harder steels structurally free carbide. It was plain, therefore, that what might be called a critical microscopical point had been reached, and corresponding critical points in the physical and mechanical behaviour of the metal might well be expected. But he did not see that it taught anything about the underlying cause of the changes. It agreed with Prof. Arnold's assumption, as it would with any other assumption which would recognise a microscopical critical point at about 0·89 per cent. of carbon.

The conclusions which he had reached, after several years of investigation, were remarkably concordant with Prof. Arnold's in all other points with which he had dealt concerning the micro-structure of steel; but they differed greatly from them with regard to the nature of the constituents of hardened steel. He did not find any such sharp demarcation between the steels containing struc-

Mr. Sauveur. turally free iron and those containing structurally free carbide. For if it be true that a steel with 0·89 per cent. of carbon was made up wholly of the composite constituent, according to his investigations the same was true of a steel with 0·80 per cent. of carbon, and of one with 0·95 per cent. of carbon. At 0·89 per cent. carbon, the saturation point of iron for the carbide Fe_3C had not yet been reached; it could still structurally assimilate more carbide. The difference between the composite constituent of a steel containing 0·95 per cent. of carbon and the same constituent of a steel with 0·80 per cent. was that the former was less structurally diluted with pure iron. So that the formula of the sub-carbide might be, with just as much reason, and following Prof. Arnold's reasoning, $Fe_{22}C$ or $Fe_{26}C$.

It was stated that since the whole bulk of a steel containing 0·89 per cent. of carbon was composed of the composite constituent; at a high temperature it must be entirely made up of the sub-carbide, *i.e.*, that a certain mass of sub-carbide would yield an equal mass of the composite constituent. This, indeed, was far from being so, for if Figs. 6 and 21, Plate 4, illustrating the structure of a steel containing 0·38 per cent. of carbon were referred to, it would be seen that in the hardened sample the sub-carbide occupied 69 per cent. of the total area, while in the normal sample the composite constituent amounted to only 36 per cent. The figures had been obtained by actual measuring with a planimeter.

The examination of nearly one hundred samples of hardened steel of all grades showed, without a single exception, that a steel containing more than 0·20 per cent. of carbon when quenched at a sufficiently high temperature, did not contain any structurally free iron, while according to Prof. Arnold's theory, this could only be the case when the metal contained 0·89 per cent. of carbon. If it were a sub-carbide of iron, therefore, which was retained in the quenched steel, by sudden cooling, his chemical composition, far from being constant, must vary according to the amount of carbon present in the steel. More than that, it must vary in the same steel according to the quenching temperature. He had tabulated the structural composition of a steel containing 0·21 per cent. of carbon and heated to a uniform temperature of 970° C., from which it was slowly cooled to the quenching temperatures indicated.[1]

[1] These bars were treated by Mr. Henry M. Howe, in connection with his investigations concerning the hardening of steel. "The Hardening of Steel," by H. M. Howe. Iron and Steel Institute, August meeting, 1895.

Mr. Sauveur.

TABLE SHOWING THE STRUCTURAL COMPOSITION OF A STEEL CONTAINING 0·21 PER CENT. OF CARBON AND QUENCHED AT VARIOUS TEMPERATURES.

Quenched at ° C.	Position of the Quenching Temperature with Regard to the critical Points.	Constituent of hardened Steel Prof. Arnold's Sub-carbide.	Structurally Free Iron.	Composite Constituent.
		Per Cent. in Volume.	Per Cent. in Volume.	Per Cent. in Volume.
880	Above Ar_3 (?)	100	..	..
714	Between Ar_3 and Ar_2 .	75	25	..
650	Between Ar_2 and Ar_1 .	30	70	..
575	Below Ar_1	..	80	20

When quenched at 880° C., the steel was wholly made up of Prof. Arnold's sub-carbide; at 714° C., it contained only 75 per cent. of it; at 650° C., 30 per cent.; and at 575° C. it entirely disappeared. No correction being made to change the percentage in volume into percentage in weight, the composition of the sub-carbide should be therefore at 880° C. $Fe_{107}C$, at 714° C. $Fe_{80}C$, and at 650° C. $Fe_{32}C$, which was absurd. It had been found by Mr. Osmond[1] that a steel containing only 0·14 per cent. of carbon, when quenched at 1,340° C., contained hardly any free iron; at 1,000 C. nearly half of the mass was structurally free iron; at 770° C. it amounted to about 75 per cent.; at 670° C. it occupied a still larger proportion of the total area. A steel containing 0·45 per cent. of carbon, Mr. Osmond had shown, when quenched at 1,000° C., contained no structurally free iron; at 730° C. it had a network of free iron. The structure of the hardened sample of the 0·38 per cent. carbon steel did not agree with his observations, or, as far as he knew, with those of other investigators. A steel of that class, quenched at such high temperature (1,000° C.) should not exhibit any structurally free iron. Such a structure as that shown could not be very hard, as the relatively large soft areas would easily be scratched by a needle.

Mr. RALPH G. SCOTT, of Leeds, thought the solution of the problems occurring in the production of iron and steel would be rendered easier by every addition to knowledge of the structure of the material and the changes which it underwent in passing from the high temperature at which it was worked to the low temperature at which it was used. He asked how the molecule — Mr. Scott.

[1] Bull. della Soc. d'Encouragement pour l'Industrie Nationale. May 1895.

Mr. Scott. of $Fe_{24}C$ could be represented, whether there was any known inorganic compound analogous to it in composition; and if there was any warrant, from a chemical point of view, for considering the proportions of iron and carbon, represented by the formulas $Fe_{24}C$ and $Fe_{10}C$, definite compounds and for calling them carbides. The origin of the different forms of carbon in No. 6 steel, Table II, p. 131, did not seem to be explained by the micro-sections. If the explanation of the absence of Fe_3C in the analysis given on p. 129 was correct, might it not be proved by acting simultaneously on thin plates of No. 3 normal steel, where the carbide was wholly diffused, and No. 3 annealed where the carbide was wholly crystallized? The resulting residue would show whether there had been a secondary galvanic action in progress causing a decomposition of the normal carbide. In the annealed section there was no diffused carbide, but a large proportion of pearly constituent; and he would have expected a fair amount of Fe_3C to be found in the residue, while the analysis on p. 128 showed only a trace. There was no explanation offered in this case for the disappearance of the Fe_3C. He asked also how the free-carbon, hydrate and graphite, were differentiated in the analysis of No. 6 steel. It had been stated in the Paper that inter-crystalline and molecular cohesion were proved to be nearly equal by the tensile test of No. 1 annealed specimen, the elongation being 53 per cent. and the reduction of area 77 per cent., because the crystals in the micro-section were large and definite. He did not understand why molecular cohesion should be considered as overcome at all in this piece. The largest crystal shown in the microscopic field was about $\frac{1}{400}$-inch in cross-section, and there was no evidence that one of these crystals had been pulled apart, or that anything but the cohesion between crystal and crystal had been overcome. He did not see how the force necessary to tear apart the component molecules of a crystal could be measured.

The remarks of Prof. Arnold on the influence of annealing on mild steel might easily lead to a wrong conclusion if the fact was not constantly borne in mind that the subject treated of is the micro-structure of steel and not the crystalline structure visible to the eye on the surface of fractures. The whole area of each of the sections, shown in Plate 4, represented the enlargement of a speck about $\frac{1}{200}$ inch in diameter, and many of the lines and points shown must be less than $\frac{1}{100,000}$ inch broad. When, therefore, such expressions as "elongated masses," "carbonized areas," "large interspaces of iron" were used, it must be remembered that they were

comparative terms; and that the masses and interspaces mentioned, could only be observed under the highest powers of the microscope, after the most elaborate and careful manipulation and etching of the piece under examination. The Paper was a most valuable contribution to the subject of the constitution of steel. It remained to be proved whether the presence of other substances as they occurred in commercial steel, which could seldom be produced so pure as the material Prof. Arnold had experimented upon, had any effect upon the saturation point. It would be interesting to know whether armour-plate material, containing 3·5 per cent. of nickel, had the same saturation point as pure steel; or whether the presence of so much nickel induced the formation of another intermediate carbide. Mr. Scott.

Mr. A. VOSMAER, of the Hague, doubted whether the iron theory would ever have been brought forward had the results of Prof. Arnold's researches been known. An explanation of physical phenomena, based upon a theory of allotropy might be entertained when it was the only one that could be given, but, even before these investigations, an allotropic modification theory had not been necessary, and in the special case of steel it had indeed many points against it. If the β iron was the essential hardening constituent, he did not see why pure iron could not be hardened. It was known that even the softest irons changed their mechanical properties by water-quenching, but their hardness was not increased. If the β iron accounted for the hardness, he could not understand why none but the carbon-steels were capable of being hardened? There were hard alloys of iron and tungsten, chromium, manganese, and other metals, but unless they contained carbon they could not be hardened. It had been said that the alloy of iron with 5 per cent. of osmium-iridium also took temper without the presence of carbon, but the experiment could not be accepted as true before it was repeated. It was well known that, in the tempering of tools for a certain hardness of steel,[1] the best standing of the cutting-edge was given at a certain temperature, above which the tool was spoiled. It was also known that a steel with somewhat less than 1 per cent. of carbon gave a better tool than one with more than that amount; but the reason was never known until it was explained by the micro-structure drawings given in Plate 4. The best method for revealing the secrets of "taking temper," and all its complicated consequences was study Mr. Vosmaer.

[1] "On the Mechanical and other Properties of Iron and Steel in connection with their Chemical Composition," by A. Vosmaer. London: E. and F. N. Spon.

Mr. Vosmaer. of the other physical properties in conjunction with microscopical research. Recalescence, magnetic permeability, hysteresis, &c., could also be used to check the results of other experiments.

Mr. Wallis-Jones. Mr. REGINALD WALLIS-JONES could, from the results of recent experience, fully confirm the remarks of Dr. Anderson as to the necessity of hammering the electrically-welded portion of iron or steel after welding. It was not possible otherwise to realise the excellent results which were attainable by this process as shown in the tests[1] conducted by Dr. Kennedy in 1890. For wrought-iron the ratios of the strength of the welded to that of the unwelded bar were:—

	Per Cent.
For bars 2 inches diameter	82
" " 1½ inch "	84
" " 1 " "	87
" " ¾ " "	98
" " ½ " "	95
Mean ratio	89

The ratios of strength of weld to solid bar in the case of the Bessemer-steel specimens were:—

	Per Cent.
For bars 1 inch diameter	92·0
" " ¾ " "	97·5
" " ½ " "	100·0
Mean ratio	96·5

In the tests[2] made by Messrs. Kirkaldy & Sons on 1⅛-inch round bars of "Farnley Iron," the mean ratios of 91·9 per cent. and 89·3 per cent. welded to solid had been obtained in the cases of electrically-welded and hand-welded samples respectively, showing that the electrically-welded bars were stronger than those welded by hand. With reference to the welding of copper, the following Table showed the results of three tests of unwelded wire and five tests of welded wire he had made in a cable factory. He had used a soft copper wire No. 10 S. W. G., the weld being made in an automatic electric wire-welder. This apparatus had an automatic switch which cut off the primary current of the transformer as soon as the metal to be welded had been "shut up" to a given

[1] Report of Dr. A. B. W. Kennedy to the City of London Contract Corporation, 3rd April, 1890.

[2] Report of Sir Frederick Bramwell to Mr. Ewing Matheson, 26th April, 1890.

predetermined amount. The burr formed by this process of welding was removed by filing, so that the diameter of the weld was the same as that of the wire itself. All the specimens had broken at the weld:— Mr. Wallis-Jones.

Unwelded.		Electrically Welded.	
Breaking Weight.	Elongation.	Breaking Weight.	Elongation.
Lbs.	Per Cent.	Lbs.	Per Cent.
500	38	410	20
500	..	470	22
510	41	440	23
..	..	400	18
..	..	370	11

The mean ratio of strength of welded to unwelded copper wire was 83 per cent. With soft copper, therefore, a good weld could be obtained; with hard drawn copper, however, it is not possible to obtain such good results, as shown in the Table. It had been stated by Prof. Arnold that in iron and steel below 0·45 per cent. of carbon iron was the chief partner of the group; and above 0·45 per cent. steel was the chief partner, and that the qualities of iron in the former and steel in the latter might be expected to predominate.

He had experienced great difficulty in one or two cases in welding steel of a comparatively low carbon. A typical case was the following: A galvanised steel wire 0·100 inch in diameter, having a high breaking strain in the unwelded state, viz., about 1,460 lbs., and giving in analysis—

	Per Cent.
Carbon	0·570
Silicon	0·014
Sulphur	0·022
Phosphorus	0·042
Manganese	0·511
Nickel	trace
Zinc	0·602
Iron	97·894
	99·655

Twelve samples of this wire had been welded under identical electrical conditions and with the same settings on the automatic welder; after welding, the portion for about ½ inch on each side of, and including the weld, had been annealed by passing a current through the wire and raising it to a dull-red heat for

Mr. Wallis-Jones. about thirty seconds. In spite of every care, the best weld would only give a ratio of strength of the welded to unwelded portions of 68·4 per cent., and the mean of twelve welds only gave a ratio of 57 per cent., and these results were in spite of the fact that, as far as carbon was concerned, the steel border-line had not far been passed.

Another case which had come under his notice was that of a No. 4 S. W. G. galvanised wire described as "Basic iron." It had stood all the necessary mechanical tests, viz., ten twists in 6 inches and an average breaking strain of 33 tons per square inch of sectional area. In this case the welds had been extremely difficult and at times impossible to make under conditions which had previously been successful in dealing with wire used for the same purpose and which had stood the same mechanical tests.

Mr. Winder. Mr. B. W. Winder, of Sheffield, remarked that although the separation of combined carbon into graphite frequently took place in steels containing over 0·90 per cent. of carbon it seldom occurred with a lower percentage, affording strong corroboration of the saturation point at 0·89 per cent. Experiments he had recently conducted had shown that in cementation up to this point the carbon was much more readily taken up than when the percentage was greater. It had been shown by Prof. Arnold that the elimination of graphite could be produced by annealing only; but the annealing process, to which he had subjected his samples, was much more severe and prolonged than was generally resorted to in the ordinary working of high-carbon steels, as, for instance, file steel. The files were raised only to a full-red heat and then allowed to cool gradually in the furnace; but in this process no graphite was set free, although, in some cases, the annealing process was repeated three times. The time had, therefore, to be determined during which it was necessary to retain the files at a temperature of, say, 1,000° C. before this dissolution took place. High-carbon steels when rolled at a low temperature were frequently spoilt by nearly the whole of the carbon separating out as graphite during the rolling into the finished size, especially in three-square and small flat sizes. This separation had generally been admitted to be caused by the rolling, but the observations of Prof. Arnold lead to the consideration whether the heating in the furnace, and especially if the material be laid in a slow fire, would not tend to increase the liability to decomposition. This could only be solved by experiment, but it was of great moment to workers in hard steels; as, although a decomposed steel, or black steel as it was generally called, hardened and lost its

graphitic appearance on hardening, it did not make nearly so good a finished article, as always on annealing (even slightly) the "black" again immediately re-appeared, and the wearing capabilities of the material were inferior to those of undecomposed steel. In annealing files soft places were often caused by the temperature of the furnace rising slightly above the normal, causing the files to lose their carbon by being transferred to the iron bar on which they rest during the time they are in the annealing furnace, and at this point the hardening was not so complete as it was in the rest of the file. By bringing this matter so clearly forward, Prof. Arnold had rendered steel-makers a great service and given them a wide field for investigation, as, although he had mentioned file-steel for example, with knife-blade steel and razor steel, especially when rolled hollow, the thin part of the blade was liable to decompose slightly, but quite sufficiently to render an excellent steel capable of producing blades of only medium quality with little power of retaining a keen cutting-edge. Mr. Winder.

Mr. CHARLES J. BAGLEY observed, with reference to Mr. Wrightson's Paper, that the experiments made at the Moor Steelworks, Stockton-on-Tees, under Mr. Wrightson's directions, had been undertaken to ascertain if mild steel, of weldable quality, followed the same law as cast-iron in the physical changes which took place in passing from the condition of solid to liquid. Two-inch cubes of steel had been placed gently, by means of a pair of tongs, on the surface of molten steel, poured into an ingot mould and in a very liquid condition. In the first instance the cube had sunk out of sight, but reappeared on the surface in about thirty seconds; and after some difficulty, owing to the heat, it had been picked out, although almost unrecognisable. There could be no doubt, however, that it was the identical piece which had been put in, as it was picked out with a pair of tongs, which proved that it was solid, and there could be nothing solid on the surface of the metal except this piece of steel. Several cubes had been tried in the same way with the same result; in each case they had been wasted beyond recognition. Later, a further set of cubes had been treated similarly in a bath of steel not so intensely hot. These pieces had first sunk, and after about thirty seconds had risen to the surface, and had been picked out promptly. Although considerably wasted, they had been still of a cubical shape, with the corners rounded. This experiment had been repeated several times with the same results, proving conclusively that low-carbon steel followed the same law as cast-iron, in first sinking and then rising Mr. Bagley.

Mr. Bagley. above the surface. The latter phenomenon, as the Author had explained, must be due to expansion, causing the cube to become considerably less dense than the liquid, and thus proving that the assumption that dilatation was continuous and uniform during the passage from the solid to the liquid was erroneous. The amount of carbon in this steel was between 0·14 per cent. and 0·18 per cent.

Mr. Stansfield. Mr. ALFRED STANSFIELD remarked that the formation of ice had previously been brought forward in accounting for the welding of iron; and that there was a close analogy between a piece of red-hot steel and a freezing salt solution. When a solution containing less than a certain amount of salt froze, pure ice was first formed. This separation of ice free from salt increased the amount of salt in the fluid residue; and, when it had attained a certain density, the residual liquid solidified as a whole without further change of composition. The mixture of salt and water which was of the density to solidify as a whole had been called the cryohydrate. It had the same composition and freezing temperature whatever the strength of the original solution; so that a solidified salt solution containing less salt than the cryohydrate would consist of crystals of pure ice in a matrix having a constant composition. A salt solution of the same composition as the cryohydrate would, when frozen, be perfectly homogeneous, and one containing more salt would consist of crystals of salt embedded in a matrix of the same cryohydrate. It had been shown by Prof. Arnold that a quickly cooled steel containing less than 0·9 per cent. of carbon consisted of crystals of iron embedded in a matrix which had a definite composition, that when the carbon reached 0·9 per cent. the whole steel consisted of this matrix, and that further additions of carbon caused the appearance of the carbide of iron Fe_3C. The crystals of pure iron corresponded to the pure ice in the salt solution, the steel matrix of constant composition which Prof. Arnold called $Fe_{24}C$ corresponded with the cryohydrate, and the Fe_3C to the embedded crystals of salt. Steel did not really become liquid at the recalescent temperature (650° C.), but freezing was a change from one molecular condition to another, and such a change was known to take place at recalescence; while steel above this temperature was so soft as to possess in part at least many of the properties of a true liquid.

There was yet another point of resemblance. Pure water had a definite freezing point at 0° C.; the molecular change in pure iron probably took place at about 850° C. When salt was added to water its freezing point was lowered, the cryohydrate freezing

as a whole at a temperature considerably below 0° C.; and in the same way, in steel containing 0·9 per cent. of carbon the molecular change had been lowered from 850° C. and merged into the carbon change point at 650° C. He did not wish to push this analogy too far; there were several points to be worked out before it could be applied exactly; for instance, was the carbon dissolved as Fe_3C, as the analogy might at first sight suggest, or was it dissolved as carbon and only combined with iron to form the carbide on its release from solution, just as salts took up water of crystallization? The many points of similarity between salt solutions and steels led him to expect that Prof. Arnold's $Fe_{24}C$ would, like the cryohydrate, turn out to be not a chemical compound after all; the solidified cryohydrate having been clearly shown to be merely an intimate mixture of salt and ice and in no sense a chemical compound. In any case the connection was one which all investigators of the constitution of steel would do well to bear in mind. Mr. Stansfield.

Mr. C. Humphrey Wingfield asked whether the spiral spring Mr. Wrightson had used to produce *Fig. 1*, p. 166, was in tension throughout the range A D. Referring to the fall at the point E, if the immersed part of the ball melted there would be no buoyancy to support it, and the apparent weight would increase as shown, even if the density of the solid part of the ball were not increased at all. He asked whether it was not possible that the fall from D to E was due to partial melting of the immersed portion and consequent loss of buoyancy; if so, the experiments would not be inconsistent with continuous dilatation until the ball was actually melted. Mr. Wingfield.

Prof. Arnold, in reply to the correspondence, had read with pleasure the remarks of Prof. Behrens, whose labour on the nature of double carbides would, in the near future, be of great service to steel metallurgists. The comments of Prof. Le Chatelier, on the separation of graphite from steel, deserved careful consideration from those investigating this important practical question, which he fully admitted was imperfectly understood. The experiments of Mr. B. W. Winder had shown: 1. That in steels containing carbon beyond the saturation point, the separation of graphite could be produced at will by finishing the rolling of steels for files or razors at a low-red heat. If finished at a fair-red heat such separation did not take place. Figures had been obtained by Mr. Winder, from which it seemed that the carbon separating as graphite was only that known to be the carbon of super-saturation. To quote an example from a Paper read by Mr. Prof. Arnold.

Prof. Arnold. Winder in 1891, a very hard file steel, melted from converted Swedish iron, was found to contain 0·76 per cent. of graphite and 0·88 per cent. of combined carbon.[1] 2. That for the separation of graphite by annealing a high temperature approaching 1000° C. was necessary, no graphite separating on or below the saturation point. 3. That the separation was preceded by a mobilisation of the surplus Fe_3C into large masses, thin membranes of Fe_3C seemed to resist decomposition. Sometimes worm-like masses of graphite formed a continuous track in the centre of large spaces of free iron, presumably originally carbide. 4. That in very high carbon steels practically the whole of the carbon could be separated as graphite. Calculations had been made by Mr. Hibbard from which he had concluded that the carbon in hardened steel had a tensile strength of about 39,000,000 lbs. per square inch, and other calculations on a similar basis he had stated yield imposing results. With this latter idea he fully agreed, though not exactly in the sense employed by Mr. Hibbard. The remark that he did not attribute any of the properties of steel to its molecular conditions suggested that his deductions had not been fully comprehended, which, whilst pointing out the great effect of variation in crystalline structure, nevertheless attributed the specific properties of hardened steels to the molecules of an attenuated carbide as opposed to those of an alleged allotropic modification of iron. He was aware that physicists would object to the length of the bars upon which the magnetic experiments were made; but, as he had pointed out, comparative and not absolute experiments were sufficient for his purpose. There was practical difficulty in drawing super-saturated pure carbon steels into wire without breaking, without surface decarbonization, and without separation of graphite. As Dr. Hopkinson had said, other elements greatly affected the magnetism of steel, especially tungsten, but how was it possible to scientifically investigate their action if the magnetic properties of pure iron and carbon steels were not known? The researches on the magnetic properties of steel to which Dr. Hopkinson had referred were not satisfactory from the point of view of applied metallurgy, since the specimens were often designated by such vague terms as piano-wire, hard steel, medium steel, etc. He had at last succeeded in obtaining a set of bars practically without manganese, and containing only about $\frac{1}{10}$ per cent. of total impurity. Only from a base line of the magnetic properties of iron and carbon steels could the properties of the triple alloys produced by nickel,

[1] Journal of the Sheffield Metallurgical Society, Vol. i. p. 82.

Prof. Arnold.

chromium and tungsten be accurately determined. He could not see what analogy could be drawn from an alloy of iron with 25 per cent. of nickel, when quenched carbon steels were the subjects under discussion. He was, however, fully prepared to admit that the change taking place at $A_r 3$ was, as suggested by Dr. Hopkinson, analogous to a change of water into ice, a view entirely opposed to the ideas of Mr. Osmond and Prof. Roberts-Austen, inasmuch as such change was not defined in classical treatises as allotropic. He fully agreed with Mr. Howe's statement that no novelty was attached to the theories discussed by that gentleman in 1890. He merely claimed to have experimentally demonstrated for the first time on a set of pure iron and carbon steels, the substantial accuracy of the sub-carbide theory, and to have determined by correlation of microscopic, magnetic, and thermal measurements, the composition of the sub-carbide. The remarks of Mr. Howe concerning the imperfections of his (Prof. Arnold's) hardened samples were inaccurate; and the statement that he, although able to harden steel from a temperature of 1000°, lacked the skill to do so from a temperature of 700° was absurd, as was the statement that on heating steel the sub-carbide or martensite progressively absorbed the iron of the ferrite, the fact being that the sub-carbide diffused into the iron when, under the influence of temperature, the two substances became sufficiently viscous. That a mechanical diffusion of the sub-carbide necessarily altered its composition was untenable. It was suggested by Mr. Howe that he had confounded the methods of annealing. Some confusion certainly existed, but not on Prof. Arnold's part. The process described was in general use in England for annealing steel castings. The lack of correlation between the micrographs and the electrolysis of super-saturated steels was explained in his reply to Mr. Osmond. It was asserted by Mr. Howe that Dr. Sorby's results proved that steel was saturated when it contained 0·54 per cent. of carbon. As he had in his possession the whole of Dr. Sorby's research series he was in a position to speak on this matter with some authority. The sample in question was a Bessemer steel ingot, high in manganese and silicon, which, even after hardening, still contained free iron. In order to accurately measure the influence of carbon on iron, he had employed steel as nearly as possible free from impurity, yet Mr. Howe, because the results obtained on a highly impure Bessemer steel did not agree with those he (Prof. Arnold) had obtained, assumed that the latter were wrong. His deductions were based strictly on correlation; had the sub-carbide theory been capable of proof by a single

Prof. Arnold. method of observation it would have been unnecessary to have carried out the investigation on five different lines of attack, and all that Mr. Howe's criticism seemed to aim at, was to prove that the results obtained by any single method did not prove the accuracy of his conclusions. This was exactly the position he (Prof. Arnold) had taken up from the first. That which Mr. Howe had termed the "second line of evidence" seemed to be of considerable strength, because Mr. Howe admitted that the "phenomenon was hard to interpret" and "seemed to support Prof. Arnold's theory." Finally, he had concluded that such support was "equivocal" and that "the phenomenon might be due to any one of a variety of causes." The nature of the "reflection" which led him to regard the data as "equivocal" had not, however, been stated by Mr. Howe, nor had he divulged "any one of the variety of causes" to which it might be due. His curves of annealed steels were "gravely suspected" by Mr. Howe, that it was "improbable" that as the carbon increased from 0·89 per cent. to 1·47 per cent. the metal would become more ductile. Such suspicions could not alter mechanical facts. The increasing ductility of annealed steels above the saturation point had been conclusively proved to be due to a separation of graphite. His data on this matter were not of the scanty nature alleged by Mr. Howe. On three distinct series of steels the curves had been continued up to nearly 2 per cent. of carbon. He ventured to quote the following typical result:—

IRON AND CARBON STEEL NO. 7*a*.

Combined carbon	0·08 per cent.
Graphite	1·70 „

The micro-structure was almost entirely of iron crystals mixed with graphite, together with a few isolated patches of plates of Fe_3C—

Elastic limit	7·08 tons per square inch.
Maximum stress	18·46 „ „
Elongation per cent. (2 inches) . .	32·50 per cent.
Reduction of area	39·20 „

The remarks by Mr. Jenkins were purely of a theoretical character, but unfortunately steel was a rebellious material, which often set the laws of theoretical physics at defiance. He noticed two inaccurate statements made by Mr. Jenkins. First, that he (Prof. Arnold) had admitted the existence of allotropic modifications of iron. The existence of such modifications he was unable either to deny or to affirm, but he had consistently denied the

existence of an allotropic modification of iron of diamond hardness Prof. Arnold. which had been consistently affirmed by allotropists. Second, it had been asserted by Mr. Jenkins, that he admitted Ar3 to be constant in amount and independent of the carbon change. He had repeatedly stated exactly the opposite, specifically pointing out the perturbation in the thermal curve due to the fact, several times emphasised that in low-carbon steels a portion of the carbon change invariably takes place at Ar3. This matter, however, did not at all affect the vital part of the curve immediately below and above the saturation-point. With regard to the suggestion of Mr. Kreuzpointner, of a possibility that under stress steel may change from a crystalline to an amorphous condition, such a view was absolutely negatived by ample microscopical evidence in his possession. The crystals of iron belonged to the cubic system, but at the point of rupture of structural steels in tension they would be found to have drawn out into needles.

In reply to Mr. Metcalf, he would point out that he employed the drastic process used for annealing brittle steel castings to obtain a maximum recrystallization. This process, though undoubtedly very beneficial to steel as cast, was, on the whole, as Mr. Metcalf had pointed out, injurious to forged steels. With reference to Mr. Metcalf's remarks about the influence of temperature on carbides, he would find on reference that Abel, Barus, Osmond and others had amply proved that far below a red heat, the carbide of hardened steel returns entirely to the form of Fe_3C. He was prepared to admit that Dr. Müller's proposal to call the constituent corresponding to the composition $Fe_{24}C$, an alloy could be supported by cogent theoretical arguments, nevertheless the term sub-carbide was practically more convenient. With reference to the hardness of Fe_3C, this he had found difficult to determine with minute plates, but, judging by its properties *in situ* tested with emery-grains, the mass hardness seemed greater than was generally supposed. He had obtained mineralogical hardness with quenched 0·9 per cent. steel equal to that of quenched 1·2 per cent. steel, nevertheless when used as a drill the hardened sample of the latter steel resisted the "letting down" action of the heat of friction much longer than the former, which would be seen on reference to the experiments of Prof. Ripper.

With reference to Mr. Osmond's remarks concerning the electrolytic isolation of the carbides, he admitted that well above the saturation-point, where a cellular structure was formed, they were not satisfactory; but here, as he had pointed out, the matter

Prof. Arnold. was undoubtedly much complicated by the formation of secondary couples, particularly when graphite was present, that substance forming an excellent kathode. The same remark applied to free carbon or to hydrate of carbon. His experience indicated that the extent of the hydration bore some relation to the compactness of the mass adhering to the bars. He could not quite follow Mr. Osmond with reference to the suggestion that if the laminæ would take an opposite electro-position to the crystallised carbide they would also do so *inter se.* The surplus carbide, however, had been free and semi-liquid at a temperature of 1000°, whilst the diffused carbide of the dark areas was comparatively finely divided, and had been deposited from the decomposition of the sub-carbide at 650° C. The delicate researches of Mr. Andrews had shown that minute differences in chemical composition, or even of crystalline stress was sufficient to cause metallic substances to take opposite electro-positions in dilute acids. He was fully prepared to discuss with Mr. Osmond the possibility that the surplus carbide differed in composition from the laminæ; but would point out that the microscopical evidence was altogether opposed to such a view. He had numerous sections in which the laminæ and the surface meshes were continuous and absolutely indistinguishable, microscopically. If the surplus carbide were so readily decomposed, as Mr. Osmond had suggested, surely some indication of this would be shown on etching the micro-sections. The statement, that he had copied the method employed by Mr. Osmond and Mr. Werth and published in 1885, was inaccurate. He had evidence to prove that the method used in the carbide determinations made by Mr. Read and himself was in constant use in the laboratory of the Sheffield Steel and Iron Works in 1881. In his criticisms of the mechanical tests, a grave fundamental fallacy underlay Mr. Osmond's arguments, namely, he ignored effects due to structural changes and argued as though the materials under discussion were homogeneous solids.

The criticism of Mr. Osmond as to his micro-sections was unfortunate. Whilst Prof. Arnold had been endeavouring for years to obtain pure iron and carbon steels, and had at last reduced the impurities to 0·1 per cent., no such considerations troubled Mr. Osmond. His medium 0·45 steel contained 0·35 per cent. of manganese; his hard 1·25 steel contained 0·35 per cent. of silicon. Of the powerful influence of these elements on the structure of hardened steels Mr. Osmond was evidently unaware. It was true that as the saturation point was approached from either side, the mixture of the constituents became so

intimate that it was difficult to distinguish them microscopically in hardened steels. For this reason he had tested the matter well above and well below the saturation point, employing a high temperature to produce complete solution, if possible. Had he employed the lower temperature of say 700° C. which he could have done legitimately, he might have registered results far more favourable to his theory. The diffusion of the sub-carbide into the viscous iron did not, however, at all effect his theory, because, before the carbon change point at Ar 1 was reached, the constituents of steel had resumed their separate areas. Had the carbon been in mere solution such would not be the case. A reference to No. 332 section in Mr. Osmond's series, would show that after being heated to 825° C., and quenched at 690°, or 20° before the carbon change-point at Ar 1 commenced, the iron and the sub-carbide had segregated in an unsaturated steel containing 0·45 per cent. of carbon, notwithstanding the fact that the metal was impure with manganese. The low-power photographs of Mr. Osmond were absolutely useless as far as hardened steels were concerned, and his high-power photographs did not bear out the statements made in his criticism. For instance, his 0·45 steel quenched at 1,225° showed distinctly the presence of a light and a dark constituent. On quenching his 0·14 steel, Mr. Osmond had said that no sub-carbide was present; it would be interesting to know what had become of the carbon. Mr. Osmond's photograph of this steel quenched at 1,000°, showed a structure consisting of light and dark constituents; but to it he would not appeal, because the preparation of the sample was evidently faulty. Such a steel properly prepared under the conditions named, consisted of small crystals of iron, and dark diffused blurs of sub-carbide around the areas formerly occupied by the normal carbide. The fact that Mr. Osmond, in tracing the distribution of martensite (sub-carbide) and his highly apocryphal constituents sorbite and trootsite, could not refer to his photo-micrographs, but constructed diagrams, was strong proof of the fact that it was only by patient, direct drawing from the microscope, that the structure of steel could be accurately delineated. Mr. Osmond's theory of the pressure of solution broke down in face of the fact, that, whilst a steel containing 1·8 per cent. of carbon could be taken up to 1000° and cooled in air without depositing graphite, the same steel heated to 1000° for a prolonged period and very slowly cooled, deposited practically the whole of its carbon as graphite. The elaborately formulated magnetic arguments of Mr. Osmond involved the initial fallacy that a plastic mass of steel at a red heat, must necessarily possess the Prof. Arnold.

Prof. Arnold. same capacity for magnetism as a crystalline mass at the ordinary temperature, and no ingenuity of argument would alter the fact that a bar of quenched high carbon steel would vibrate in 17 seconds under the influence of the earth's magnetism, whilst a bar of quenched manganese steel would not vibrate in a thousand years. It was stated by Mr. Osmond, that quenched high carbon steel was an alloy of α- and β-iron; now β-iron maintained its allotropic condition owing to the presence of dissolved carbon. The molecular configuration presented by an alloy of two allotropic modifications of the same element, one charged with dissolved carbon and the other free from it, must be unique, and to the Author incomprehensible. However, Mr. Osmond's argument resolved itself into the following. α-iron had no capacity for permanent magnetism; β-iron was absolutely non-magnetic. Quenched high carbon-steel consisted of an alloy of α- and β-iron. Therefore, the high capacity of quenched high carbon-steel for permanent magnetism was due to α-iron, which was absurd. He did not agree with the florid statements of Mr. Osmond in the opening of his criticism, that the divergence of opinion between them was infinitely great, and he would also suggest that an *ex parte* assertion, that Prof. Arnold's theory was impossible, was not calculated to encourage that amicable and impartial consideration of the subject from which alone its true solution would proceed.

The remarks of Mr. Sandberg on steel rails deserved the consideration of engineers engaged in railway practice. It was noticeable that Mr. Windsor Richards, Mr. Sandberg and himself, looking at the question from three distinct points of view, all deprecated high-carbon rail steel. With reference to Mr. Sandberg's remarks about silicon, he would ask that gentleman to suspend judgment on the matter until he had read the Paper which he presently hoped to have the honour of laying before the Institution on the remarkable influence of silicon in throwing out sulphide of iron in its most deadly mechanical form between the crystals of steel. As to the influence of silicon *per se* on steel rails he agreed with Mr. Sandberg. He dissented entirely from Mr. Sauveur's statement that it was unnecessary to discriminate between the dark areas of normal steel and the clearly laminated areas of annealed steels. The former were not pearlite, inasmuch as they did not present interference colours, and the mechanical effect of the pearly constituent differed largely from that of the diffused carbide, a fact clearly shown by the mechanical curves. The Table given by Mr. Sauveur taught an important lesson, namely, that before the carbon change point at A_r1 had taken place, he found in a steel contain-

ing 0·21 per cent. of carbon a volume of 70 per cent. of structurally free iron. What stronger evidence could be adduced in support of his (Prof. Arnold's) contention that the carbon was not in solution but mainly in isolated areas as a definite sub-carbide? However, speaking broadly, Mr. Sauveur's Tables and somewhat laboured arguments on formulas had little bearing on the Paper or theory it contained. Would it not have been better had Mr. Sauveur stated that the metal which he called "a steel, containing 0·21 per cent. of carbon" was abnormally impure material containing an unstated amount of sulphur and phosphorus, together with no less than 1·2 per cent. of manganese and 0·3 per cent. of silicon. To compare such a steel, containing at least 1·6 per cent. of impurity, with his own containing only 0·17 per cent. was obviously inadmissible. Surely Mr. Sauveur must be aware that even in unhardened steels the amount of manganese present would *per se* at once lower the saturation point to about 0·7 per cent. of carbon, and in hardened steels much lower, owing to the formation of a double sub-carbide of iron and manganese, the diffusive power of which was much more marked than that of the single carbide, whilst its capacity for segregation was much less marked. Unlike Mr. Sauveur, he had had no difficulty in determining the fact that iron containing 0·96 per cent. of carbon was super-saturated. It was useless for Mr. Sauveur to urge that there was no structurally free iron in his (Prof. Arnold's) quenched sample containing 0·38 per cent. of carbon. The comparative softness of the material was clearly shown by the compression test, and, as Mr. Sauveur surmised, the metal could be machined. He had several sections from oil-quenched gun-steel test-pieces, in which, in spite of the 0·5 per cent. of manganese present, the structurally free iron emeshing the dark double sub-carbide, was most clearly marked. In stating that his observations did not agree with those of other microscopists, Mr. Sauveur was also in error. Quenched sections of steel of medium hardness had been described by Prof. Behrens as consisting of small hard grains surrounded by a soft iron network. Upon the accuracy of Prof. Behren's results Mr. Osmond had thrown doubt, they were nevertheless identical with those Prof. Arnold had obtained. Prof. Arnold.

In reply to Mr. Scott's question with reference to the graphic constitution of $Fe_{24}C$, it was altogether premature with the present imperfect knowledge of attenuated alloys of definite composition to attempt to represent such complex molecules. With reference to the mechanical test of No. 1 steel annealed, when the intercrystalline cohesion was faulty the metal would break along the

Prof. Arnold. original facets of the crystals; in this case, however, the metal drew out almost to a point and the crystals at the point of fracture resembled elongated cones. He considered the analogue upon which Mr. Stansfield proposed to found a theory for steel to be more tenable than that of sulphur proposed by Prof. Roberts-Austen or that of pitch suggested by Mr. Jenkins. Unfortunately the microscopical, thermal and magnetic data could not be satisfactorily explained on the basis so ably put forward by Mr. Stansfield. The mere solution of Fe_3C could not give the decisive thermal effects noted. The dissociation and solution of the carbon should yield homogeneous microscopical fields in the cold metals, differing only in depth of colour, as he had already pointed out. He was gratified that an engineer like Mr. Vosmaer, who had devoted special study to the subject of iron and steel, was inclined to accept the deduction he had drawn from his experiments.

Mr. Wrightson. Mr. WRIGHTSON explained, in reply to Mr. Wingfield, that the spiral spring was in tension between the points A and D, *Fig. 1*, p. 166. The slight fall from D to E was no doubt due to a slight loss of mass by surface-melting, and any vertical measurements after the point D was passed were useless for the calculation of densities. The steeper descent from E to F was due to the rapid and entire melting of the ball which joined the bath of molten iron and became of the same density, or that which is represented by the line of liquid volume B F. The ball had disappeared but molten iron of the liquid density exactly occupied its place, and therefore no buoyancy was registered. All that could be stated was that in the plastic condition the altitude of point D indicated the density and that the altitude of B or F indicated the fluid density; while the altitude of point A indicated the solid density. If the last were given the diagram enabled the other two to be calculated.

When it was found that certain uniform physical changes, such as those at the recalescent and welding temperatures occurred in iron containing the smallest amount of carbon or other impurities, it was reasonable to consider that such changes were inherent to the iron apart from chemical considerations. If it were further found that these physical changes still occurred, though probably in a somewhat modified state, when carbon was added, it appeared that such allotropic changes should be regarded as important factors in the result, and that chemistry could not possibly account for all the phenomena of hardening or tempering. As to the physical effect of pressure, if a piece of steel were highly heated and then plunged into water, the outer skin suddenly hardened and

contracted upon the still heated interior, causing great internal pressure under which the cooling took place. Could it be supposed that this purely physical effect, of no chemical origin, had to be neglected in considering the causes of hardness? These varied phenomena could not be accounted for by either chemical or physical considerations alone. The physical changes in the element must be studied along with the contemporaneous chemical changes in the compound, and in the study of their combined action only could be found the true solution of problems which belonged to both departments of science. Mr. Wrightson.

10 December, 1895.

SIR BENJAMIN BAKER, K.C.M.G., LL.D., F.R.S., President, in the Chair.

The discussion upon "The Physical Properties of Iron and Steel" occupied the evening.

17 December, 1895.

SIR BENJAMIN BAKER, K.C.M.G., LL.D., F.R.S., President, in the Chair.

(*Paper No. 2857.*)

"The Design and Testing of Centrifugal Fans."

By HAMMERSLEY HEENAN, M. Inst. C.E., and WILLIAM GILBERT, Wh.Sc., Assoc. M. Inst. C.E.

THE experiments recorded in the Paper have recently been made by the Authors upon several of the best known varieties of fans in use, and had for their object:—

(1) To determine the best shape of the fan-blade and fan-case, in order to secure a minimum expenditure of power when producing any given output of air, *i.e.*, to find the best type of fan;

(2) The standard type being selected, to obtain data whereby the proper diameter of the standard fan and its most economical speed could be determined for any required output of air at a given pressure.

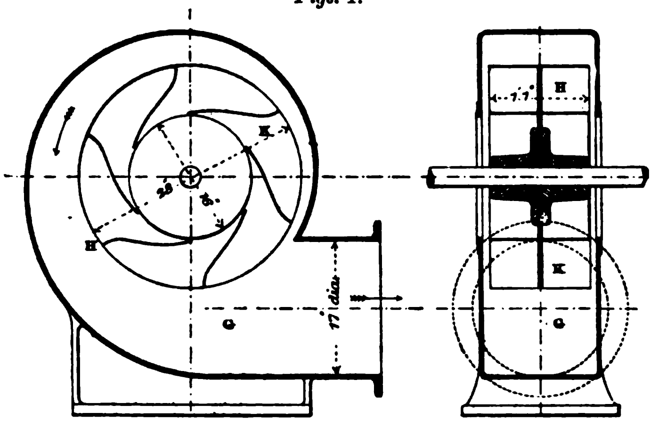
The experiments proved that a fan with a few simple blades gives the best result, provided the form of blade and the dimensions of the casing are designed to suit the kind of work required. Fans of more complex design have too large an internal resistance to give the highest mechanical efficiency, although they may have to be used if high pressures are essential. In the study of air-propellers there is great difficulty in taking correct measurements of the pressure and quantity of the air delivered, if the velocity of the stream is at all great. From this cause, experiments, apparently conducted with the greatest care, have often shown a fan under test to give an efficiency of over 100 per cent., and therefore, general statements of the efficiency of fans should be received with great caution. Considerable doubt also appears to exist as to the proper method of comparing the performance of different fans. To meet the first difficulty, the Authors carefully tested and re-arranged the measuring instruments until results known to be consistent were obtained. Particulars of these tests will be given later in the

Paper, and the Authors now proceed to give an outline of the manner in which the performance of any fan should be tested and put on record.

PRELIMINARY.

A common type of blast-fan, *Figs. 1*, consists of a centre or drum H K fitted with, say, six blades, revolving within a casing. The air enters through the centre of the sides of the drum, and is discharged by the outlet G. Suppose such a fan to be running at a constant speed, taking air from the atmosphere and discharging through a delivery tube H K, Figs. 2, Plate 1, having an outlet at K, the area of which can be varied. Let the fan-centre be assumed

Figs. 1.



Scale, ¼ inch = 1 foot.

28-INCH BLAST FAN.

to be 28 inches and the delivery-tube 17 inches in diameter. Let there be two water-gauges, A and B, one of the branches of each of which is connected to a pipe soldered into the side of the delivery-tube in the gauge A; the other branch is attached to a pipe passing to the centre of the delivery-tube, the end being suitably bent to face the stream. The pressure in the pipe C (inductive action being prevented) will be that due to the compression of the air only, but the pressure in the pipe D will be that due to pressure and velocity combined. Hence, the gauge A, which indicates the difference between the pressures in the pipes C and D, will record the pressure due to velocity only. The compression of the air can be measured by the second gauge B.

First, if the outlet be closed so that no air is delivered, the

gauge A will remain at zero, since there is no flow of air through the tube, but the second gauge B will indicate a considerable compression, about $11\frac{1}{2}$ inches of water, if the tip speed of the fan (supposed to remain constant throughout the experiment) is 12,000 feet per minute. Next, let the end of the delivery-tube be opened to give, say, half the area of the outlet K. The fan now passes a considerable quantity of air, about 8,000 cubic feet per minute, and, on account of this flow, the velocity-gauge A will indicate nearly $1\frac{3}{4}$ inch of water. The compression, as shown by the gauge B, will have fallen to 8 inches. During the time the outlet of the delivery-tube was closed, with no air being delivered, the efficiency was of course 0. But when the fan is passing 8,000 cubic feet of air, under a compression of 8 inches of water, the efficiency reckoned on the compression alone will be about 66 per cent., 15 HP. being required to drive the fan. When the outlet of the delivery-tube is fully opened, the fan delivers freely to the atmosphere, and gauge B shows that the air is under no compression whatever. But the amount of air discharged has increased to about 13,700 cubic feet per minute, and the passage of this quantity of air through the delivery-tube shows a velocity on the gauge A of nearly 5 inches of water. Since the air is not compressed but merely expelled at atmospheric pressure, the efficiency now reckoned on the compression is 0. The compression alone enables the fan to do useful work in overcoming resistance. The energy due to velocity will be almost always wasted in shock.[1] Since then the efficiency for no delivery, or for maximum delivery, is zero, there is necessarily an intermediate delivery at this particular tip-speed of 12,000 feet per minute, for which the efficiency, reckoned on the compression produced on the air, is a maximum. This may be exhibited, together with the other results of the preceding experiment, as follows.

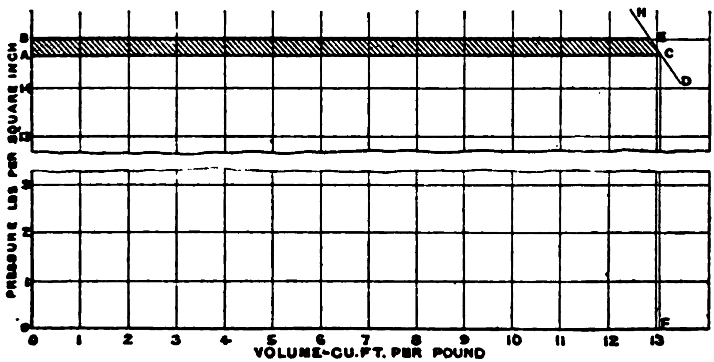
Take two axes, Ox Oy, at right angles, Fig. 3, Plate 5, and for any size of opening at the end of the delivery-tube set off along Ox the volume delivered by the fan, as Oa for the half area of the opening mentioned, when the delivery was 8,000 cubic feet per minute. On the ordinate $a\,c$ set up $a\,d$ (8 inches), the compression produced at that delivery, $a\,e$ to represent the brake HP.

[1] It may be as well to point out that there are cases where the velocity energy may be as important as the compressive, for instance, where large quantities of air have to be exhausted from a building. Here both gauge-readings would probably be exceedingly small.

required to drive the fan, and $a\,g$ the calculated efficiency. Mark off other values of the compression, brake HP., and efficiency at O, where the delivery is zero, at b where the delivery is 13,700 cubic feet per minute, and for any other intermediate points where readings have been taken. The curves shown in the Fig. may then be drawn. It will be seen that the compression curve $h\,b$ is remarkably like the external characteristic curve of a separately excited dynamo, if for volume be read current and for compression electromotive force; whilst the variable opening at the end of the delivery-tube corresponds with the resistance of the external circuit. The internal resistance of the fan represents the armature resistance. The point b is equivalent to the dynamo short-circuited.

These curves, for any tip-speed, may be called the characteristic curves of the fan for that speed. If the curves are drawn for tip-

Fig. 4.



speeds of 5,000 feet, 6,000 feet, &c., up to 14,000 feet per minute, they form a complete picture of the capabilities of the fan.

Referring to Fig. 3, Plate 5, it will be seen that the HP. required to drive the fan increases with the area of the outlet. The use of a small outlet restricts the delivery of the fan, and hence the HP. required is less.

To plot the efficiency-curve on Fig. 3, the useful work done by the fan must be known. This, for any given values of the compression and discharge, has been estimated as follows:—Take the case of a fan drawing air from the atmosphere and delivering into a reservoir under pressure. Let O F and F C, *Fig. 4*, represent the original volume in cubic feet, and the pressure in lbs. per square foot, of 1 lb. of the air which enters the fan, and let A B represent the pressure, by water-gauge, produced by the fan.

The air will be compressed along the adiabatic D C E and expelled from the fan into the reservoir along the line E B, the shaded area B A C E being the diagram of work done. The diagram is drawn for a pressure, A B, of 10 inches of water, which is seldom reached in large fans; therefore, for all practical purposes the work done may be taken as the rectangle AC AB, the corner cut off by the compression-curve being neglected. Hence, the work done by the fan, in foot-lbs. per lb. of air entering the inlet is:—Volume of air in cubic feet per lb. × increase of pressure in lbs. per square foot, and where

v = volume of air entering the fan in cubic feet per minute,
i = pressure, or compression, measured in inches of water,

a pressure of 1 inch of water being equivalent to 5·2 lbs. per square foot,

$$\text{the horse-power of the fan} = \frac{V \times 5\cdot2\,i}{33{,}000} = \frac{V \times i}{6{,}352} \quad . \quad (1)$$

It appears reasonable to assume that the compression-curve is adiabatic, for since the air passes through the fan in a very small fraction of a second, there would be no time for the abstraction of heat by the casing. However, the exact class of this curve does not much affect the calculation. The efficiency of the fan is taken to be its effective work, divided by the power expended in driving the fan shaft for a given time. For an increase of pressure of 10 inches of water there is a rise of temperature of $3\frac{3}{4}$° F. due to the adiabatic compression. If the conditions, however, are such that the air regains its original temperature before being used, the volume will diminish about $\frac{3}{4}$ per cent. and the effective work of the fan be correspondingly reduced.

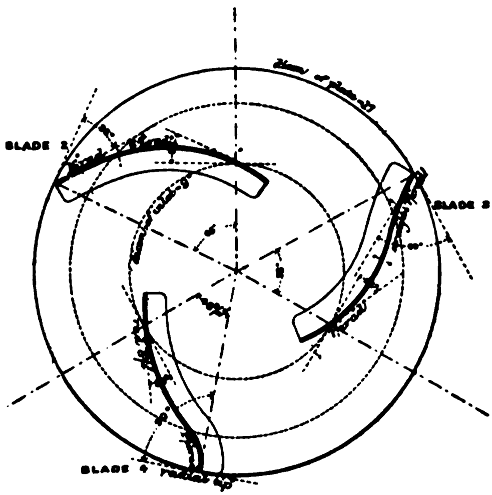
Variation of the Shape of the Blades.

Having established a method of representing the performance of a fan, the change in the characteristic curves when the shape of the blades is varied may be examined. Suppose a fan of the type shown in *Figs. 1* be selected, and in succession with blades No. 2, No. 3, and No. 4, *Fig. 5*. The angles which the edges of the three blades make with a plane perpendicular to the radius at the point considered, are 25° at the inlet and 35°, 60°, and 90° respectively at the outlet, the fan-centre being 17 inches in diameter and 8 inches wide. As far as possible in each case the air passes on to the blade without shock. The angle between the blade

and the circumference of the inlet circle is obtained by compounding the radial velocity of the flow of the air through the drum, with the tangential motion of the air relatively to a point in the circumference of the inlet circle. This angle would not be the same for all speeds and discharges, but a slight variation of the required angle is of little importance. Figs. 6, 7 and 8, Plate 5, give the characteristic curves of these blades for a constant tip-speed of 12,000 feet per minute. The "total gauge" in the Figs. is the gauge-reading due to pressure and velocity combined, from which the total efficiency is derived.

It will be seen that the radial-tip blade No. 4 gives the best

Fig. 5.



Scale, ½ full size.

result. Blade No. 3, with a tip-angle of 60°, is not greatly inferior; but blade No. 2, having a tip-angle of only 35°, gives a low efficiency, owing to the rapid drop of the compression-curve as the discharge of air increases.

Experimental Apparatus.

The arrangement of the apparatus whereby the actual measurements of the brake HP., volume of air discharged, compression and speeds were obtained in the case of fans of moderate dimensions is shown in Figs. 9, Plate 5. The fan is driven from the countershafting B B, which derives its motion from

a spherical steam-engine C. The outlet of the fan is connected by short circular iron delivery-tube with a boiler flue EE, 2 feet 6 inches in diameter and 18 feet long. At the centre of the flue a partition F is fitted, to which can be attached a series of plates having circular orifices of various sizes, varying between $4\frac{3}{4}$ inches and 18 inches in diameter. Each orifice in turn acts as a constant resistance to the fan. A well-cut outlet of known diameter is placed at the end of the flue G, where the velocity of the air was measured by an anemometer. This opening was made much larger in diameter than the outlet of the fan, in order to avoid injuring the anemometer by a violent current of air. This outlet and the levers whereby the anemometer was moved over all portions of the outlet, the instrument being kept truly perpendicular to the axis of the flue, are shown in the end elevation. Very consistent air-readings were obtained in this manner. The pulley H driving the fan, was not keyed to the shaft, but was driven by it through an Emerson power scale, a form of transmission dynamometer in which the moment of the driving-effort is balanced, through a system of levers, by a pendulum moving over a graduated scale. A speed-counter records the revolutions of the shaft. This instrument was tested by the Authors at the conclusion of the experiments by applying a band-brake to the pulley H, and on comparing the two readings the difference was almost inappreciable. A tachometer K, coupled to the countershafting B B by a spiral spring, enabled the speed of that shafting to be regulated and the proper tip-speed to be approximately maintained by the man in charge of the engine. A small hand-counter, carried in a sliding frame L and applied when necessary to the fan-spindle, enabled the exact number of revolutions per minute of the fan to be obtained.

Measurements of the pressure and the velocity of the air-stream were taken at the section M M of the delivery-tube. The velocity varied greatly in different positions on the same cross-section of the tube. Readings were taken at several points in a cross-section by means of two gauges, each of which could be traversed over a diameter at right-angles to the other. It was afterwards found unnecessary to measure the velocity, since it can be readily calculated when the discharge, as measured by the anemometer, is known; but at the same time the velocity, as measured by the velocity-gauge, gives a check on the anemometer readings.

If the cross-section of the delivery-tube be divided into a number of imaginary areas, and the square root of the mean

gauge-reading for each area be multiplied by that area and by a suitable constant, then these results added together give the total discharge of the fan.

Table I illustrates the variation of velocity referred to; it gives the gauge-reading due to velocity in four positions along a diameter of the cross-section of the delivery-tube where the gauge was fitted, as taken simultaneously.

TABLE I.

Position.	Gauge-reading due to Velocity.	Air-Velocity.	Distance from Centre of Tube.	Remarks.
	Inches.	Ft. per Second.	Inches.	
1	3·20	118	+ 3·1	Diameter of Delivery Tube 8·9 inches.
2	2·88	112	+ 1·1	
3	1·85	90	− 1·1	
4	1·70	86	− 3·1	

The measurement of the compression presented some difficulty, owing to the fact that the air flowing across the end of the side-gauge, caused a large amount of induction; a vacuum being often recorded where a pressure was known to exist. The Authors are indebted to Professor W. C. Unwin, F.R.S., for the information that a plate placed across at the end of the tube would prevent this inductive action. The form of side-gauge used is shown in Fig. 28, Plate 5. It was tested by the Authors in a manner subsequently described, and was found to give very correct results.

To draw the characteristic curves from the experimental results, seven resistance-plates, as before mentioned lettered A B C D E F, with graded circular orifices, were arranged to fasten on to the centre F of the boiler-flue. Two observations were taken with each plate of the discharge, the compression and the horse-power supplied to the fan at, or near, each of the tip-speeds 5,000 feet, 6,000 feet and 12,000 feet per minute. Owing to the slip of the belt on the fan-pulley at high speeds, and from other causes, it was impossible to maintain the tip-speed quite constant for any length of time; hence the following laws were assumed, and were afterwards proved to be true:—

$$\left.\begin{array}{l}\text{(1) The air-discharge varies as the speed} \\ \text{(2) The gauge-reading varies as the (speed)}^2 \\ \text{(3) The brake HP. varies as the (speed)}^3\end{array}\right\}\text{ for a constant resistance.}$$

The results were then plotted thus:—On a sheet of sectional paper the square root of the compression, for any given plate, was plotted, with the speed recorded by the counter. Since the compression for a constant resistance varies as the square of the speed, the points fall approximately on a straight line. These lines were obtained on the same sheet for each one of the plates A B C D E F, and from them for any intermediate speed the corresponding square root of the compression could be obtained. In the same way, the speed was plotted with the air-discharge, and with the cube root of the horse-power given to the fan.

Using these curves of water-gauge, air-discharge and brake HP., characteristic curves were drawn for tip-speeds of 5,000 feet, 6,000 feet, 7,000 feet, to 12,000 feet per minute. The method is as follows (see Fig. 3, Plate 5):—Suppose the tip-speed be 12,000 feet per minute. Taking two axes O x, O y at right angles and using O x as a volume scale, set out O1, O2, O3 . . . O6 to represent the volume passed through the plates A, B, C . . . F at this constant tip-speed, taking the data from the air-discharge curve. Draw the ordinates 1A, 2B, &c., and mark off, on the ordinate representing each resistance-plate, the compression and the brake HP. taken from the interpolation-curves previously mentioned. From the curves plotted, points on the efficiency-curve can be calculated and the curve may be drawn on. The gauge-reading due to velocity is calculated, and added, as also the total efficiency, that is, the efficiency reckoned on the gauge-reading due to velocity and pressure combined. From the method of obtaining the curve, it will be seen that if OA represents any volume delivered by the fan, then $a\,d$ will be the corresponding compression, $a\,e$ the brake HP., and $a\,d$ the efficiency for that volume and pressure. The point g might be taken as the best working point on the efficiency-curve, so that the best output for this fan, at the tip-speed of 12,000 feet per minute, is 8,000 cubic feet at a compression of 8 inches of water. A comparison of the characteristic curves of two different types of fans, at the same tip-speed, gives an excellent idea of their relative merit.

All the tests recorded of Heenan fans were made on centres with parallel sides, and with the proper clearance in the case to allow the air to flow uniformly from the fan-centre at all portions of its circumference.

With the apparatus described, tests were made on several fans of various types. Fig. 10 shows the characteristics obtained at a tip-speed of 12,000 feet per minute from a fan 16 inches diameter, having tapering side-plates. The diameter of the inlet was $5\frac{7}{8}$

inches, the widths of the fan-centre at the inlet and outlet being $7\frac{1}{4}$ inches and $1\frac{1}{8}$ inch respectively. This fan ran in a concentric case, the clearance being $1\frac{1}{8}$ inch. The compression obtained with the outlet closed was 9·4 inches against 11·2 inches of blade No. 4, *Fig. 5*. Further, the compression obtained fell off very rapidly with the increase of output, so that the working compression would not be more than 8 inches. The efficiency was also low. Fig. 11 shows the characteristic obtained at 12,000 feet per minute from a fan-centre 12 inches in diameter, the maximum width being $2\frac{3}{4}$ inches. The centre rotated in a whirlpool chamber of 23 inches diameter. The centre was of cast-iron and the tip angle of the blades was 30°. Of course, the efficiency measured would not be representative in so small a fan, but the maximum compression did not exceed 4·4 inches. The effect of a whirlpool chamber is to produce a very quiet fan.

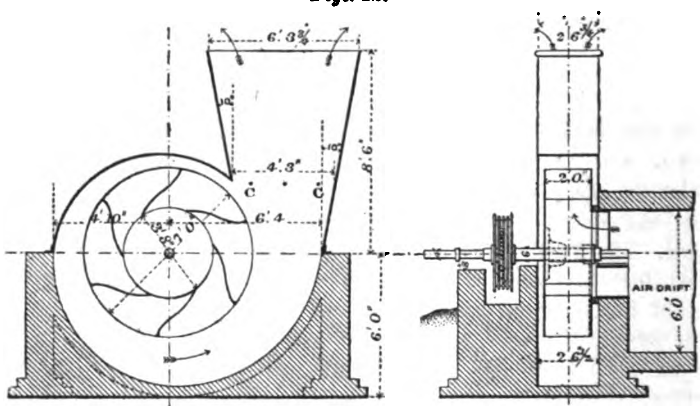
Test of a Mine-Ventilating Fan.

Of the tests of mine-ventilating fans made by the Authors, that of a fan supplied to the Parkend Colliery Company, South Wales, is selected for illustration, as giving a fair average result. The managing director of the company, Mr. F. H. Deakin, Assoc. M. Inst. C.E., placed the fan at the Authors' disposal for this test. The fan, in connection with the approach tunnel and ventilating shaft of the mine, is shown at *Figs. 12*. The fan-centre is 7 feet in diameter and 2 feet wide. The upper portion of the case and evasee chimney is built up of wrought-iron plates, the lower portion being formed in brickwork $4\frac{1}{2}$ inches thick. The fan was driven by a horizontal non-condensing engine, the cylinder being $12\frac{1}{8}$ inches in diameter and of $17\frac{3}{4}$ inches stroke. Cotton ropes were used for driving, two only being in operation at the time of the experiment. The engine was an old one, and was not supplied with the fan. The boiler-pressure averaged 40 lbs. per square inch.

To provide a variable resistance for the fan, three 9-inch by 3-inch planks were placed across the mouth of the air-drift, and boards nailed to these planks restricted the flow of air to the fan more or less as required. The folding doors at the top of the ventilating-shaft were open during the whole of the test. A tachometer driven by a belt from the fan-shaft enabled the approximate speed of the fan to be judged and regulated by the man in charge of the engine; the number of revolutions in a two-minute reading being obtained by means of a hand-counter

held to the fan-shaft. The engine-speed was obtained by a small counter applied to the shaft in the same manner. The air-discharge was measured by an anemometer held at the top of the fan-chimney, a staging being erected for that purpose. The area of the top of the chimney was divided into eight equal rectangles by means of wires tightly stretched across, and the anemometer, attached to a small iron tube, was held for a quarter of a minute in each division. In this fan the flow of air was fairly uniform over the whole of the outlet-area of the chimney, but in some cases, where the fans were run slowly for experimental purposes, guide-vanes had to be fitted in the base of the chimney to secure the result mentioned. Four degrees of opening were arranged at the adjustable orifice, and for each of these readings were taken with tip-speeds of 4, 5, 6, 7

Figs. 12.



Scale, ⅛ inch = 1 foot.

7-FOOT HEENAN MINE-FAN, PARKEND COLLIERIES.

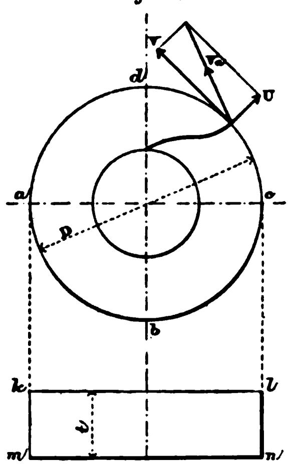
and 8,000 feet per minute. The duration of each reading was two minutes, and it was taken twice. The observer with the watch was stationed in the engine-room, and signalled by an electric bell to the observers at the anemometer and the fan-counter. Indicator diagrams were taken from the ends of the engine-cylinder during each reading. The vacuum produced by the fan was measured by a side-gauge, placed in the air-drift close to the fan inlet, and a pipe led from this tip to a water-gauge placed on a table outside. Three side-gauges, C C, *Figs. 12*, were also placed at the root of the chimney, so that the vacuum produced, and, consequently, the efficiency of the chimney, could be determined. It was found that the vacuum was practically the same at each of the three gauges, so that only one of them was read.

Figs. 13 to 16, Plate 5, give the characteristic curves obtained from this test. The dotted lines *e e* correspond with the resistance offered when the choking-boards were removed and the fan took air from the mine only. The maximum efficiency increases with the speed, and is 67·2 per cent. at a tip-speed of 8,000 feet per minute, and 70·3 per cent. at 9,000 feet per minute. The fan was designed to pass 20,000 cubic feet of air per minute, which it was estimated would require a 3½-inch head, as measured by the water-gauge, to force through the mine, the speed being 300 revolutions per minute. Referring to the ordinate *e e*, which represents the mine on the characteristic curve for 7,000 feet tip-speed, or 318 revolutions per minute, the compression is 3·45 inches of water, and the discharge per square inch of centre-section is 11·6 cubic feet, corresponding with a total discharge of 23,150 cubic feet per minute, so that the fan is amply large for the work.

Comparative Output of Different Fans.

Several fans of the same type but of different sizes having been tested, the dimensions of the fan to which the output is proportional can be ascertained. Suppose two fans of different dimensions, but running at the same tip-speed, are fitted with delivery tubes, as in Fig. 2, and let the area of the resistance-plates be so adjusted that the velocity of the air through each fan-centre is the same. The air-velocities in the respective fan-centres may be conveniently compared by dividing the discharge of each fan by the circumferential area of the fan-centre, that is, by the area of a rectangle the adjacent sides of which are the circumference *a*, *b*, *c*, *d*, and the width *t*, *Fig. 17*. This would give the radial component of the velocity of discharge, V, from each fan. Since the fans are of the same type, and run at the same tip-speed, the total pressure generated by each fan will be the same. It will be greater than the total water-gauge in each delivery-tube by the amount necessary to overcome the resistance of the fan itself to the passage of the air through it. The loss of pressure in the fan will be partly due to skin-friction,

Fig. 17.



but in a far greater degree to shocks and eddies caused by abrupt changes in the velocity and direction of flow. The velocity of the air being the same at corresponding points in each fan, the pressure lost in each fan will be the same, since the number of abrupt changes was the same. The available pressure in each delivery-tube will therefore be the same. It may therefore be concluded that if two fans of different dimensions but of a similar type are running at the same tip-speed and give the same pressure, the velocity of the air through each fan-centre will be the same; and hence the volume of air discharged per minute by each fan will be proportional to the circumferential area of its revolving centre, or more conveniently to the area of the rectangle *k*, *l*, *m*, *n*, *Fig. 17*, formed by the diameter and width of the fan. This may be termed the centre-section. The volume discharged per unit of area of centre section should be the same for all fans of the same type, if the tip-speed and pressure are the same. Hence in plotting the characteristics for different fans, if, instead of the actual volume delivered, there be plotted the volume discharged per square inch of centre-section of the fan in question, the characteristics should be identical.

This is the manner in which the characteristic curves were usually plotted, and Figs. 18, 19, and 20, Plate 5, show them for 18½-inch, 28-inch, and 48-inch Heenan fans, at a tip-speed of 6,000 feet per minute, the centre-section areas being 171, 364 and 726 square inches respectively. The brake horse-power is not divided by the centre-sectional area, but the efficiency is known, and the brake horse-power can be readily calculated from the data. From inspection of the three characteristics just referred to, it will be seen that the working output of these fans, at a tip-speed of 6,000 feet per minute, might be taken at 12 cubic feet per square inch of centre-section per minute. The compression would be 2 inches of water, and the efficiency at this delivery, reckoned on the compression alone, 56 per cent. To save weight and space, the working point is selected where the output is 10 per cent. to 15 per cent. greater than at the point of maximum efficiency. Referring to Fig. 19, it will be seen that the maximum efficiencies of the 28-inch fan are 73 per cent. and 80·5 per cent. respectively, reckoned on the compression and the total gauge-readings.

Dimensions of a Fan for a Given Output.

Supposing a set of the characteristics mentioned to be drawn for each tip-speed between 5,000 feet and 14,000 feet per minute; then if a fan be required to give any number of cubic feet of air

per minute at a given reading of the water-gauge, its dimensions can be obtained as follows. Find for what tip-speed the required compression is obtained near the point of maximum efficiency. This speed being selected and the working-point on the efficiency-curve fixed, the efficiency and volume per square inch of centre-section are known. This fixes the brake horse-power and also the area of the centre-section, since the total delivery is given. A suitable ratio is maintained between the diameter and the width of the fan-centre, and the revolutions are fixed to give the required tip-speed. In designing a fan, it would usually be arranged to work at a point of the efficiency curve corresponding with *g*, Fig. 3, Plate 5; hence the fan would have a working output of rather more than half the volume of air it would discharge if freely open to the atmosphere. It may be noticed that measurements of the water-gauge reading and the volume, &c., of a fan when discharging freely into the atmosphere, are not of much assistance in determining the real value or efficiency of the fan, although such figures are often given.

Since the fans are usually designed to work at one point on the characteristic curve, it would obviously be convenient to have a single set of curves which would give the information required about this working point for all tip-speeds. These curves may be obtained as follows. Let an ordinate be ruled on the characteristic curve, at the best working point, for several of the tip-speeds between 5,000 and 14,000 cubic feet per minute. Tip-speeds are then plotted as abscissas, with the compression and volume per square inch of centre-section as ordinates, the value of the efficiency reckoned on the compression only being written on the volume curve. A set of such curves for the design of ship-fans are shown in Fig. 21, Plate 5. The working point is not the point of maximum efficiency, but a point where the output is 10 per cent. to 15 per cent. greater than that.

As an example of their use, suppose a fan is required to pass 9,000 cubic feet of air per minute at 4 inches on the water-gauge. Referring to the Fig., it will be seen that *b* is the point on the compression-curve where the compression is 4 inches. Drawing an ordinate at this point, cutting the volume-curve at *c* and the base-line at *d*, the volume passed per square inch of centre-section is seen to be 14·6 cubic feet per minute, and the corresponding tip-speed 7,900 feet per minute.

The required area of centre-section is:

$$\frac{9{,}000}{14 \cdot 6} = 616 \text{ square inches.}$$

Assuming the ratio of width to diameter to be $w : 0\cdot18\,d$,

$$0\cdot18\,d^2 = 616;$$

hence $d = 58\cdot6$ inches, and $w = 10\frac{1}{2}$ inches.

A fan 60 inches by 11 inches would be used, and the correct revolutions are—

$$\frac{7{,}900}{5\,\pi} = 502 \text{ per minute.}$$

The efficiency taken from the curve is 56·5 per cent.; hence the I.HP. of the engine would be—

$$\frac{9{,}000 \times 4}{6{,}352 \times 0\cdot565} = 10\cdot0 \text{ I.HP.}$$

The efficiency, 56·5 per cent., is reckoned on the compression only. The total efficiency as obtained from the characteristic curve is here 66 per cent.

Test of Water-gauge Tips.

The apparatus by means of which the accuracy of the tips used for the measurement of air-pressure and velocity was tested is shown in Figs. 22, Plate 5. To test the facing-gauges, the following method was adopted: The tip was moved at a known velocity through air at rest, and provision was made whereby a water-gauge recorded the pressure thus set up. B B is a wrought-iron circular tank, 7 feet in diameter, and divided into eight compartments by vertical dash-boards C. The tip to be tested was screwed into a horizontal pipe, D E, attached to and rotated by the hollow vertical spindle of the apparatus. The top bearing of this vertical spindle was formed in a hard-wood block, a stuffing-box being used in connection with it. From the upper portion of the block a pipe led to the water-gauge. Free communication was thus established between the tip under test and the recording water-gauge. The vertical spindle was driven from a counter-shaft by a belt pulley and suitable gearing, the revolutions per minute being recorded by the hand-counter F. The tip under test was arranged to describe a circle 20 feet in circumference.

The object of the dash-boards being to prevent motion of the air in the tank, the openings in them through which the tip and pipe passed were made as small as possible. A facing-gauge G was inserted into the tank, just clear of the path of the revolving tip. No reading was detected on this gauge, so that the velocity

of the air in the tank could never have exceeded 10 feet per second, corresponding with a reading of $\frac{1}{44}$ inch. Velocities lower than this could not be read on an ordinary facing-gauge. The tip-speeds used reached 200 feet per second.

Test of Facing-Gauge Tips.

Suppose air to be flowing directly against the mouth of a tube which is connected to a gauge for the purpose of measuring the pressure produced by this velocity. The pressure, h, in feet of air, due to a velocity of V feet per second, is

$$h = \frac{V^2}{2g} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

It is required to find out if the form of the tip affects the correct registering of this amount. If h be expressed in inches of water, as it would be measured in practice, regard must be had to the relative densities of air and water at the time of the experiment.

Let h = pressure measured as a column of air, in feet,
i = the corresponding pressure measured in inches of water,
T = the absolute temperature of air in degrees Fahrenheit,
and h_b = the height of the barometer in inches of mercury.

Then P V being equal to 53·2 T for one pound of air, where P, the pressure of the atmosphere, would be measured by a barometer, the following numerical relation is found between h and i:—

$$h = 3\cdot 91 \frac{i \times T}{h_b} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Substituting this value of h in equation (1),

$$i = \frac{V^2 h_b}{251\cdot 7\, T} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This formula gives the reading of the water-gauge due to a velocity V for any given atmospheric conditions of barometer and temperature.

For T = 60° F. and h_b = 30 inches,

$$i = \frac{V^2}{4{,}370},$$

an approximation near enough for ordinary calculations.

For instance, if the air-speed were 200 feet per second, the reading of the water-gauge due to this velocity would be—

$$i = \frac{(200)^2}{4{,}370} = 9 \cdot 15 \text{ inches.}$$

To test a tip, the water-gauge connected with the revolving-tube is observed, and the reading compared with that calculated as above from the measured velocity of the tip relative to the air in the tank. A correction must, however, be introduced for the vacuum produced by the centrifugal force of the air in the revolving horizontal tube D E.

This vacuum, estimated in feet of air-column, is

$$h = \frac{v^2}{2g},$$

when v is the speed of the tip. That this is so is mathematically proved in Appendix I. Hence, if the tip measures the pressure due to the air-velocity correctly, there should be no reading on the water-gauge for any speed (see formula (2))—the vacuum due to centrifugal force just balancing the pressure due to the velocity of the moving tip against the air. The results of the tests of three facing-tips are given in Table I, Appendix II.

Experiment No. 597 gives the result of some of the tests made on the standard facing-gauge adopted, Fig. 23, Plate 5. The reading of the water-gauge connected with the revolving tip is given in column 3, and it will be seen that a slight vacuum was indicated instead of no gauge reading. The calculated vacuum which would be due to an air-velocity equal to the tip speed used is given for each case in column 3. The slight vacuum recorded would be accounted for by the assumption that a small stream of air was carried round by the tip. The reading of the water-gauge would then be due to the relative velocity of the air and tip, diminished of course by the gauge reading due to centrifugal force; assuming this hypothesis to be correct, the actual air-speed in the tank required to produce the vacuum observed, could be calculated, column 5. The maximum velocity does not reach 3 feet per second when the velocity of the revolving tip was 180 feet per second. Hence the standard facing-gauge adopted may be taken as correct. Experiments Nos. 602 and 603 were made on the bell-mouthed facing-tips, Figs. 24 and 25. The vacuum here recorded in the revolving tube was somewhat greater than in the case of the standard facing-gauge for corresponding

tip speeds; this, however, is to be expected, since the speed of the air-stream carried round with these tips would be greater.

It follows, therefore, that the form of a facing tip, whether conical, parallel, or bell-mouthed, for the measurement of velocity, is not of material consequence.

Test of Side-Gauge Tips.

From what has been remarked, it will be seen that when a side-gauge is rotated, the water-gauge reading will be the vacuum due to the centrifugal force of the air in the rotating tube, plus the vacuum produced by the inductive action of the air flowing across the face of the tip. It is the amount of the latter action that is to be determined by experiment.

The results of tests on three forms of tips used as side gauges are given in Table II, Appendix II. Experiments Nos. 604 and 605 were made on the side tips, Figs. 26 and 27. The vacuum recorded in the revolving tube is given in column 3, and in column 4 is shown the calculated vacuum which would be produced in the revolving tube by the action of centrifugal force. The difference between these readings, as given in column 5, is the vacuum produced in the tube by inductive action at each speed. It follows, therefore, as tabulated in column 6, that if these tips be used to measure the pressure in a stream of air flowing through a pipe, by placing them at right angles to the direction of flow, the pressure recorded will be less than the correct amount by a quantity equal to about 45 per cent. of the gauge-reading, which would represent the velocity of the air-stream in question, column 6. Small differences in the shape of the tips might considerably affect the result. Experiment No. 606 was made on the standard side-gauge adopted, shown at Fig. 28. The vacuum recorded in column 5 would be accounted for by an air-velocity in the tank itself, of about the same amount as that produced by the bell-mouthed facing-gauges (experiments Nos. 602 and 603). It appears reasonable to assign the small reading obtained to that cause, and to assume that the inductive action is completely neutralised by this gauge. A side-gauge will indicate correctly even if the direction of the flow of the air makes a considerable angle with the face of the gauge. This can be proved by moving the gauge in the air-stream from side to side. For low velocities, such as obtain in mine-drifts, the form of gauge is unimportant since there is practically no indication on the gauge due to velocity, or any inductive action to be guarded against; but in the case of

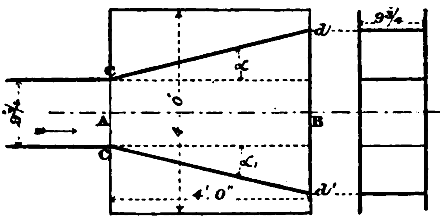
blast or cupola fans the conditions are very different, and a correct side-gauge is essential.

EFFICIENCY OF EXPANDING CHIMNEYS.

The Authors made a series of tests, which are not, however, to be regarded as exhaustive, to obtain the efficiency of an expanding chimney for various angles formed by the sides of the chimney. The kind of apparatus used is shown in *Fig. 29.*

Air is passed from a fan into a delivery-tube $9\frac{3}{4}$ inches square. The end of the tube terminates in two hinged flaps C d and C^1 d^1, which can be set at any desired angle to the centre-line of the tube. The flaps are also $9\frac{3}{4}$ inches wide and move between two side-plates 4 feet long. The side-plates can be set up to form an air-tight joint. A facing-gauge was introduced at A to measure the velocity of the air, and, in addition, a side-gauge was applied

Fig. 29.



to measure the vacuum existing at that point due to the action of the chimney. The vacuum produced was uniform over the whole of the section at A, but the velocity of the air varied considerably. The area of the section was divided into six equal parts, and a velocity-gauge was read in each part.

The velocity at the outlet is obtained from a comparison of the inlet and outlet areas:—

Let v_i denote the air-velocity at the inlet A,
and v_o the air-velocity at the outlet B.

The pressure at the outlet is atmospheric, and the vacuum at A which balances the loss of the head of the air due to velocity, as it passes from the inlet to the outlet of the chimney, is evidently—

$$\frac{v_i^2 - v_o^2}{2g} \text{ feet head of air.}$$

Comparing the calculated with the measured vacuum, the

efficiency of the chimney, for any particular angle, is obtained. A selection of the results is given in Appendix III.

When the chimney is moderately close to the fan-blade, the air may not fill the chimney uniformly but flow closer to one side, leaving a partial vacuum on the other side, so that there is an inrush of air at that place. This is particularly the case with chimneys of wide angle when the velocity of the air at the outlet falls below 8 feet or 10 feet per second. Experiments 58 to 64 were made with the side plates adjusted so that the air escaped uniformly over the outlet area, and an increase of efficiency was the result. A good speed for the air at the chimney outlet is 20 feet per second. In this case, the loss on the water-gauge as velocity in air,

is $$\frac{(20)^2}{4,375} = \text{about } \tfrac{1}{10} \text{ inch.}$$

It will be seen from the Table that the efficiency with this speed at the outlet varies between 0·43 for 6° opening, and 0·42 for 15°.

The conclusion to be derived from the tests of expanding chimneys is that in designing a fan-chimney, the angle on each side may be as much as 15° without loss of efficiency, but means may have to be taken to give a uniform discharge over the outlet area.

The Paper is accompanied by ten tracings from which Plate 5 and the *Figs.* in the text have been prepared.

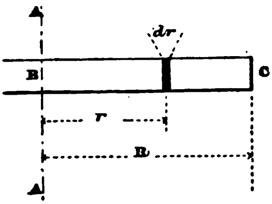
[APPENDIXES.

APPENDIXES.

APPENDIX I.

Proof that the Pressure due to the Velocity of the Tip is balanced by the Vacuum due to Centrifugal Force.

Fig. 30.



Let BC, *Fig. 30*, be a tube rotated about the axis AA; it is required to find the pressure on the end of the tube C, due to the centrifugal force of the air enclosed.

Let v be the velocity of the outer extremity of the tube C.

A = the area of the cross section of tube.

G = density of air (weight per cubic foot).

Take a lamina of the air of thickness dr at a radius r.

The volume of this lamina is A dr, its weight is G A dr, and its velocity is $\frac{r}{R} v$; hence the centrifugal force of the lamina

$$\frac{G\,A\,dr \times \frac{r^2}{R^2} v^2}{g \times r} = \frac{G\,A\,v^2}{g\,R^2}\, r\,dr;$$

hence the centrifugal force of the air in the tube—

$$\frac{G\,A\,v^2}{g\,R^2} \int_0^R r\,dr = \frac{G\,A\,v^2}{2\,g};$$

and the pressure per unit of area at the end C

$$P = \frac{G\,v^2}{2\,g}.$$

Dividing this result by the density, the pressure measured in feet of air column is obtained as under—

$$h = \frac{}{G} = \frac{v^2}{2\,g}.$$

APPENDIX II.—Results of Tests of Tips.

Table I.—Tests of Facing-Gauge Tips.

1 No. of Experiment.	2 Tip-Speed.	3 Water-gauge Reading (Vacuum).	4 Gauge-Reading due to Tip-Speed.	5 Calculated Air-Speed in Tank.	Remarks.
	Ft. per Sec.	Inches.	Inches.	Ft. per Sec.	
597	79·90	0·01	1·49	0·10	Standard facing-gauge, Fig. 23, Plate 1. Barometer = 30·17 ins. Temperature = 53° F. $i=\frac{v^2}{4,285}$
	97·00	0·03	2·20	0·65	
	114·00	0·07	3·03	1·40	
	142·00	0·09	4·70	1·50	
	172·00	0·21	6·90	2·70	
602	62·66	0·02	0·89	0·66	Small bell-mouth facing-tip, Fig. 24, Plate 1. Barometer = 29·38 ins. Temperature = 54·4° F. $i=\frac{v^2}{4,415}$
	103·44	0·15	2·42	3·24	
	132·22	0·26	3·96	4·52	
	169·50	0·42	6·51	5·60	
	180·50	0·48	7·39	5·90	
603	67·16	0·05	1·02	1·56	Large bell-mouth facing-tip, Fig. 25, Plate 1. Barometer = 29·37 ins Temperature = 57° F. $i=\frac{v^2}{4,440}$
	99·55	0·11	2·24	2·45	
	132·83	0·23	3·97	4·03	
	164·16	0·35	6·07	4·86	
	184·00	0·43	7·62	5·40	

Table II.—Test of Side-Gauge Tips.

1 No. of Experiment.	2 Tip-Speed.	3 Water-Gauge Reading (Vacuum).	4 Calculated Vacuum due to Tip Speed.	5 Vacuum due to Induction.	6 Gauge-Reading due to Induction.	Remarks.
	Ft. per Sec.				Per Cent.	
604	68·8	1·59	1·07	0·52	49·0	Thin side-tip, Fig. 26, Plate 1. Temperature = 53·2° F. Barometer = 29·47. $i=\frac{v^2}{4,395}$
	109·5	4 03	2·73	1·30	48·0	
	137·2	6·18	4·27	1·91	45·0	
	160·0	8·50	5·82	2·68	46·0	
	190·4	11·66	8·24	3·42	42·0	
605	81·8	2·24	1·51	0·73	48·0	Medium side-tip, Fig. 27, Plate 1. Temperature = 57° F. Barometer = 29·63. $i=\frac{v^2}{4,400}$
	99·8	3·20	2·26	0·94	42·0	
	113·2	4·22	2·90	1·32	46·0	
	136·8	6·40	4·25	2·15	51·0	
	167·8	9·65	6·38	3·27	51·0	
606	69 3	1·13	1·09	0·04	3·7	Standard side-gauge, Fig. 28, Plate 1. Temperature = 44° F. Barometer = 28·89. $i=\frac{v^2}{4,400}$
	81·0	1·58	1·49	0·09	6·0	
	104·0	2·58	2·46	0·12	4·9	
	127·3	3·90	3·68	0·22	6·0	
	151·0	5·57	5·19	0·38	7·3	
	187·0	8·48	7·95	0·53	6·7	

In experiments 604 and 605 a slight correction is necessary in the last column, owing to the air speed in the tank.

APPENDIX III.

Efficiency of an Évasée Chimney for various Angles.

No. of Experiment.	Angle of Sides of Chimney.	Velocity of the Air. At Inlet A.	Velocity of the Air. At Outlet B.	Observed Vacuum in Inches at A.	Calculated Vacuum in Inches at A.	$k = \frac{\text{obs. vac.}}{\text{cal. vac.}}$
10	3°	32·1	21·2	0·02	0·13	0·15
12		40·8	26·9	0·11	0·21	0·51
14		65·7	43·4	0·33	0·55	0·60
16		79·8	52·6	0·55	0·82	0·67
18	6°	32·0	15·7	0·07	0·18	0·39
20		49·2	26·1	0·20	0·42	0·48
22		67·9	33·3	0·45	0·80	0·56
24		79·0	38·7	0·67	1·07	0·57
26	9°	33·9	13·2	0·05	0·22	0·22
28		50·7	19·8	0·18	0·49	0·36
30		69·0	27·0	0·40	0·91	0·44
32		82·1	32·1	0·63	1·30	0·48
34	12°	32·3	10·4	0·07	0·21	0·33
36		50·3	16·3	0·18	0·52	0·35
38		65·2	21·1	0·39	0·96	0·41
40		80·0	25·9	0·60	1·30	0·46
42	15°	33·2	9·1	0·11	0·23	0·47
44		48·6	13·3	0·15	0·50	0·30
46		67·4	18·5	0·40	0·90	0·42
48		77·0	21·2	0·52	1·25	0·42
50	18°	31·6	7·5	0·06	0·31	0·28
52		48·1	11·4	0·18	0·50	0·36
54		63·8	15·2	0·35	0·88	0·40
56		79·0	18·8	0·49	1·34	0·37
58	6°	33·1	9·7	0·10	0·23	0·44
60		48·0	14·1	0·22	0·48	0·45
62	21°	67·3	19·8	0·54	0·94	0·57
64		81·2	23·8	0·75	1·49	0·52

Discussion.

Sir BENJAMIN BAKER, K.C.M.G., President, thought the Authors were wise in coupling together the subjects of the design and the testing of fans. He had no doubt that on going back a sufficient number of years most of those sitting at the Council table would have felt tolerably confident that they could design a fan on theoretical principles without absolutely testing; but those were days when it was thought that the pressure on a flat plate arising from a current of air could be found by the very simple and elementary process of ascertaining the height of a column of fluid corresponding with that velocity, assuming that the pressure of the current was the same as the static pressure of the column. It was also then taught that one could arrive equally easily at the corresponding pressure on an inclined surface or a curved surface with the aid of a Table of sines. But engineers had learned their ignorance since, and now knew that they could not theoretically arrive at results which would enable them to design an economical fan; and they had, therefore, to fall back upon testing. About ten years ago Lord Rayleigh had told him that he had at last arrived at a mathematical expression for the distribution of pressure on the front surface of a cylindrical thin plate, but that his powers of mathematical analysis did not enable him to find any expression for the negative pressure at the back. Since this was the case with a flat plate, it could be imagined what a tremendous complication of eddies and vortices must be going on inside a fan, and it could be easily seen that the only way in which any practical result could be arrived at was by the process of testing adopted by the Authors. In the present day engineers were not above considering slight economies, even 1 per cent. or 2 per cent. being not beneath their notice; and he had no doubt that the work done by Mr. Heenan and Mr. Gilbert would encourage the users of fans to see whether they were really getting the utmost out of them. He was sure that every one would recognize the importance of engineers taking up in detail any tests of that kind, so that they might get the utmost economy out of every part of a machine, although it might seem to be an insignificant part. He begged to move a vote of thanks to the Authors for bringing the results of their tests before the meeting. Sir Benjamin Baker.

Hon. R. C. Parsons.

The Hon. R. C. Parsons observed that some years ago he had an opportunity of making a number of tests upon centrifugal pumps at the works of Messrs. Easton and Anderson at Erith. Those experiments were carried out in the greatest detail. The laws for centrifugal pumps for water he could not but think approached very nearly those of the fans for air. Indeed the disturbances which took place in water-fans were practically the same as those which took place in air-fans, and *Fig. 5* showed the shape of the blades with which he had an opportunity of experimenting. He first tried a blade similar to that of No. 2 where the curve was continuous, instead of having a reverse curve as in the case of Nos. 4 and 3, and the results obtained were practically that the continuous curve required one shape of case, while that of the reverse curve required another shape of case, for the reason that the circumferential velocity produced by the blade in Nos. 4 and 3 was greater than the circumferential velocity in the case of the blade in No. 2. The tests made with those two fans led to the following conclusions, which were published in the Proceedings of the Institution.[1] The efficiency of the blades with the continuous curve was practically the same as those having the reversed curve, when the casings surrounding the fans were modified in the manner he had stated to suit the form of each blade. The laws which he deduced at that time were borne out by the results of the experiments, and the principles upon which they were based were fully explained in the Paper referred to. He agreed with the Authors that a spiral casing surrounding the fan was of great importance in securing good efficiency; and further, a diverging orifice, so as to reduce the velocity of the air as much as possible before it escaped, was of great advantage. The efficiency curves of the pumps that he worked out rose from zero somewhat similarly to those on the wall, but they did not dip down in the way the Authors had shown, and touch again the zero line. According to the Authors' curve, when the fan was discharging a large amount of air, it was giving no efficiency. He did not think that the Authors intended to show that, because it would indicate that it was a very bad machine for raising the wind. It might be that they only took the efficiency in the compression of the air, and did not consider that the fan was really doing any work when it was transmitting a large volume of air. He could not but think that that was an erroneous method of calculating efficiency. The most important thing in designing a fan was not

[1] Minutes of Proceedings Inst. C.E., vol. xlvii. p. 267.

so much the form of the blades, but that the air should enter the fan with as little shock as possible, and be reduced to the lowest velocity upon escaping. He thought if the Authors had graduated the entry into the fan, as shown in *Fig. 12*, as well as they had graduated the exit of the fan, the efficiency would have been considerably increased. The experiments with the facing gauges were similar to some Mr. Parsons had made years ago, and he had found that the velocity indicated in the tip gauge, as the Authors called it, was a very satisfactory mode of estimating the velocity of the water, as they had found it in the case of air. Hon. R. C. Parsons.

Mr. J. Imray desired to indicate another direction in which experiments might be conducted. The question had been almost entirely what amount of compression the fan had been able to give to the air; but he thought that a far more important point was the velocity given to the air. Nearly fifty years ago—in 1848—when he was engaged in some engineering work connected with the new Houses of Parliament, there was a long range of vault, with about 1,200 feet available, and a fan was put at one end, and the velocity of the air was ascertained. The mode adopted, which he mentioned because it might interest future investigators, was for a person at the fan to heat a piece of iron red-hot, and at a certain moment to let drop upon it, at the instant it was visible, a few drops of scent such as eau-de-cologne, whilst the person at the end of the vault with a stop-watch ascertained the time when the scent was perceptible to him. That gave a very good notion of the velocity of the air. The experiments were carried on for some time, but he regretted that he had not the data obtained from them. The best fan in respect of velocity proved to be one with plain blades inclined back at an angle of about 30° from the radius. There was no curvature in the blades. The air came in at a large opening at the centre and went out at a spiral outlet like that shown in *Fig. 12.* Another singular thing was, that although the vault in which the experiments were made had a great number of piers standing out for 8 or 10 inches; the velocity at the sides of the vault was the same as that at the centre. That was rather an anomalous result, but it was tried over and over again by the scent test and confirmed. He mentioned the matter so that future investigators who had the advantage of a long tunnel might try such experiments. Mr. Imray.

Mr. Bryan Donkin congratulated the Authors upon their interesting Paper, containing a large number of facts and very few opinions. Considering there were so very few reliable experi- Mr. Donkin.

Mr. Donkin. ments with smaller types of fans, the work was one which must have taken much time and trouble. As he had himself made a few experiments on fans, he would venture to offer some practical observations respecting them. He was rather disappointed, on reading the Paper, to find that only one type of fan was tested, viz., the Heenan, although there were, of course, various sizes tried. That was to be regretted, as a comparison with other types would have been extremely interesting. In a Paper published in the Proceedings,[1] he had eleven different types, and it would have been interesting to try Mr. Heenan's fan with them, so as to ascertain their relative merits. He quite agreed that there was a great want of a recognised standard for testing fans. Various experiments had been made, in some cases very carefully, but they all gave different results. It was desirable to have a distinct set of standard rules to follow, so that the fans might be tested on the same lines, and compared together. The question of anemometers was rather a vital point. The whole measurement of air and its velocity was based in this Paper upon anemometers, but he had not much faith in them. The best experiments were those made at Breslau in Germany, with a large air-holder, and the Commission found, as the result of a very large number of trials, that the anemometers used gave too high results, viz., from 7 to 13 per cent. in excess of the actual measurements by the air-holder. The Authors did not say what anemometers were used, what was their diameter or type, and whether they were calibrated. With regard to the gauge M in Figs. 9, Plate 5, he should say that it was too near the fan to produce very reliable results. He supposed that it would be 3 feet or 4 feet off. A considerable churning action would be going on in the fan, which would, he thought, extend to the gauge M. The Authors had also mentioned the slip of the driving straps, and if they could add some data as to the percentage of the slip in a few leading experiments, it would be useful. The Authors had said nothing about the bearings of the fans. This was rather a critical point in a fan; and it would be desirable to know the diameter, the width and how they were lubricated, because their friction entered into the question of the mechanical efficiency. He should also be glad to know whether the Authors had tried the fans with the vanes reversed, so as to compare the results when the fans were revolving in the contrary direction. It would be rather an interesting experiment, and it was one which he had himself made. Also whether

[1] Minutes of Proceedings Inst. C.E., vol. cxxii. p. 265.

any experiments had been made with straight vanes. There were one or two fans in the market with perfectly straight vanes, giving a very high efficiency. He might also ask what was the friction alone, or B.HP., of the spindle, the power of the spindle alone going at a high speed with the vanes removed. As to the number of vanes, the Authors seemed to have fixed upon six. Did they consider that the best number? Mr. Donkin considered that a greater number of vanes would have given a better result. Perhaps the Authors had made some experiments in that direction. Those described only spoke of the angle of the vanes; he should be glad to know whether experiments had been made with twelve or twenty vanes, as well as with six. Mr. Donkin.

Mr. C. H. Wingfield said the Authors had stated that "the energy due to velocity will be almost always wasted in shock." Of course, a certain amount of velocity was wasted in shock in a casing; but in fans with which he was more particularly acquainted—those used for torpedo boats—there was no casing round the fan; the boat itself formed the casing. It discharged into a large room—the stoke-hold; and the air which came out at high velocity had plenty of space and time in which to come to rest before it met the sides of the boat. In that case it would be, no doubt, correct to take the total efficiency-curve given by the Authors, which, of course, came to a very much higher amount than the efficiency-curves as drawn in hard lines. It was common to use two or more fans in one stoke-hold, and a rather interesting question then arose as to their effect upon each other. Supposing that one of them might be doing no work whatever, the whole of the work was done by the other. For instance, in Fig. 15, Plate 5, the water-gauge, when the tip speed was 8,000 feet per minute, and there was no discharge, was about the same as the water-gauge in Fig. 16, under letter B. There was thus the same water-gauge with the same fan when discharging 17½ cubic feet per square inch of centre section, with a tip speed of 9,000 feet per minute, as when the fan was discharging nothing at a speed of 8,000 feet. Therefore, supposing there were two fans of that size in one stoke-hold, one running at 8,000 feet and the other at 9,000 feet per minute, and the opening fire-grate was such that the discharge was 17½ cubic feet per centre section, one of those fans would be doing nothing at all, while the other would be doing the whole of the work. If the higher speed fan was a little increased in speed, the air began to run backwards through the suction-pipe of the other. He thought that was clearly proved by the diagrams. Another point shown by the efficiency-curves Mr. Wingfield.

Mr. Wingfield. was that if both the fans were supplied with the same steam-pipe, so that by opening one valve both fans could be regulated, they would correct each other. If one fan was blocked up in the discharge pipe, the Authors had shown that the power necessary to drive it was very considerably reduced. That had been well known, but it was brought out markedly in the diagrams. The result of any tendency to slow down in one fan was that there was a larger margin of power left to increase the velocity, which, of course, it would proceed to do; the one fan would catch up the other and they tended to keep abreast. The Authors had given an equation for the horse-power of the fan. The volume passing through the fan, multiplied by the pressure, measured in inches of water, was given as the measure of the horse-power; it varied as the volume multiplied by the pressure. The Authors had given three laws[1] which were confirmed by experiments which had been published in *Engineering*.[2] They might be taken as substantially accurate, but taking them to be absolutely correct, it appeared from the Paper that V varied as the speed of the fan, *i* varied as the square of the speed, and therefore the horse-power of the fan varied as the cube of the speed. The efficiency was measured by dividing that by the horse-power expended in driving the fan. The third law showed that that also varied as the cube of the speed, so that the numerator and denominator of the fraction varied together. So that with one of the resistance-plates, A B C &c., if the three laws given were absolutely true, the efficiency would be absolutely the same at all speeds with that particular plate. The curve showed that it was not so, but he did not quite know why. Of course it meant that there was some slight error in the "laws" or in the observations. But taking the efficiency with, say, the A and E plate of the one diagram and the efficiency with the same two plates of another diagram, he found that the ratio between them kept very constant. Then, taking Figs. 13, 14, 15, and 16, with the same fan at different speeds, and taking the highest efficiency in each, he found that the volume per square inch, at a maximum efficiency, as near as he could plot with that scale, varied directly as the speed of the tips, so that at maximum efficiency the volume varied directly as the speed of the tips. That, of course, was merely a confirmation of the first law. The effect of making the blades more radial was to make the air fly out with a higher velocity and in a more tangential direction than

[1] *Ante*, p. 279.
[2] *Engineering*, Dec. 30, 1887, p. 669, Table 5.

with sloping vanes. As it was desirable to avoid a direction of flow normal to the casing, it seemed likely that a radial blade would be more advantageous where the fan was provided with a casing, and he observed that in *Fig. 5* the Authors preferred the blade with a radial tip. Prof. Rankine, it was interesting to observe, had described and recommended the precise shape shown in No. 4 blade.[1] There were two extreme forms of fan blades or centrifugal-pump blades. One was the purely radial blade, the other was a spiral such as was used in Appold's pump. The first was a "centrifugal" pump, inasmuch as the delivery was increased by the whirling round of the fluid to be pumped. The other was a "wedge" pump, since if the air was not impeded by the casing, more would be discharged per revolution if the fluid did not whirl at all. The presence or absence of a casing and also its shape would help to determine which was best. In a casing coming near the tips of the blades it was sometimes advantageous to curl the blades forward so as to present a hollow face to the discharge-pipe, and he was not sure that this would not be the case with the Authors' fans. The Authors had dealt almost entirely with fans with flat sides, and Mr. Wingfield noticed rather a peculiar ratio between the areas. The Authors began with a small area at suction, and then suddenly jumped to about 1¾ times this area at the inner end of the tips, and then nearly doubled that area again at the outer tips. He should be glad to know if the Authors had made any experiments to show that that was the best arrangement. One fan with conical sides had been referred to, but that fan was of very unusual proportions. Taking the suction as unity, the inlet was about one and a quarter times that area, and the periphery ran down to about half of the area. It was not quite fair to take a fan of such proportions. The Authors said the efficiency was low. The diagram was drawn to half the scale of the others, so that it appeared rather low, but the efficiency was 66 per cent., and that was better than the efficiency in Fig. 6, and nearly as good an efficiency as in Figs. 3 and 7. There was reference in the Paper to the effect of a whirlpool chamber in producing a very quiet fan. The late Mr. E. A. Cowper had shown another way of doing it, which was worth noting, as an additional precaution. In the Proceedings[2] there was a sketch showing that the sharp angle between the body of the casing and the delivery pipe was one cause of the noise made by the fan, Mr. Wingfield.

[1] "Machinery and Millwork," p. 597, sec. 651.
[2] Minutes of Proceedings Inst. C.E., vol. lv. p. 155.

Mr. Wingfield. and Mr. Cowper made a fan in which instead of that sharp point he had a corner with a very large radius, and he found that he had produced a very quiet fan. It had occurred to Mr. Wingfield that the reason of the result might be that it was analogous to the action of a whistle. If the air was blowing against that sharp edge, *Fig. 12*, it had very much the same action as air in a whistle which was purposely made to blow against the sharp edge to produce a loud noise. If there was no sharp edge there was no whistle and no loud noise, aud if there was no sharp edge in the fan the result would be the same. The Authors had stated that the characteristic curves should be identical in fans of different sizes, and had given Figs. 18, 19, and 20, Plate 5, as illustrations. The characteristics in Figs. 18 and 20 were very nearly identical. The maximum efficiency was about 61 and 59. Figure 19, however, was very much higher, and there must be some peculiarity about that fan which had not been mentioned. It would be interesting to know what it was that made it so much better than the others. There was a point which often arose in torpedo boats which the Authors had not dealt with. Their designers were very often tied for room. They were told not to exceed a certain diameter in the fan, but that they should get a certain volume of air out of it; on the other hand, they were not allowed to exceed a certain speed. As far as he knew there were only two ways of meeting this. Given a fan to deliver a certain volume in a certain number of revolutions, if the blades were made more radial, the rate of revolution would be reduced. Also, if the sides were placed further apart, so as to widen the blades, they would get more. He was quite in the dark as to what the efficiency was, and should be glad if the Authors could throw any light upon the point as to which of the two methods was the best. The tip shown in Fig. 28 was new to him. He would like to know what really was the reason for that tip. If air passed at right-angles to a pipe, he thought it always caused a vacuum, but in the present case it was stated that it did not do so. A tip was shown with a very sharp edge and a particular slope at the back. Had the Authors found that that had anything to do with it? If not, it was not quite clear why, if that pipe was put just inside the wall of a building or flush with the inside of a tube, as shown in Figs. 2, it did not have an equally good effect? With regard to the gauges on Table 1, it was shown that the velocity varied enormously in different positions on a diameter. Unless the whole circle was divided into squares and every square in it was individually measured, he did not see how any accurate result

Mr. Wingfield.

could be got from measuring along diameters in which the velocity varied very largely, and then guessing what it was doing between them. Probably that was why the anemometer was resorted to. The equality between the centrifugal force and the pressure due to whirling the gauge-tips (see Appendix I) was very pretty, and he had not seen it worked out before. The Authors had explained that both ends of the pipe leading to and from the velocity-gauge were inside the discharge-tube, but in Fig. 9 only one end was shown inside. This was doubtless an unimportant omission, still it might be misleading unless attention were drawn to it. Assuming that the other end of the velocity-gauge was provided with a trumpet-tip like Fig. 28, was it always in the immediate neighbourhood of the facing tip when a reading was taken? If not, the readings would be considerably modified by the variable pressures which he understood existed at different parts of the discharge-pipe.

Mr. Schönheyder.

Mr. W. Schönheyder considered some of the fans now made vile articles, but he did not think the Authors had arrived yet at the best result, especially with regard to the shape of the vane. Fans like that in Figs. 2 must necessarily produce a high velocity of revolution in the casing, thereby inducing much friction of air against the casing. What had to be attempted was to get air through the fan as quickly as possible. The first angle of the vanes where the boss joined the shaft might be something like that shown in *Fig. 5*, No. 4, but it should be in such a manner that the edge of the vane would cut into the air which entered the fan as a knife cut into a cheese, so as neither to drive the air on one side or the other. In the next place the vanes should be so made as to give the air a gradually increasing circumferential velocity, so that by the time it left the fan it was of the same velocity as the periphery of the fan, and for that reason the blade 4, *Fig. 5*, should have been brought forward gradually more to the left so as to give the air that gradual increase of velocity. There was nothing novel in that; it was only what turbine manufacturers had practised for many years. Shock to fans was caused by giving the air a sudden reverse direction of motion, or a sudden sharp alteration in direction, and he did not think that a much better arrangement could be devised than that shown in *Figs. 12*. Taking the cross-section of *Figs. 12*, which was that of an ordinary common fan, the air which entered at an inlet at the right-hand side of the portion nearest to the top was coaxed to take a sharp angular movement;

Mr. Schönheyder. but that, of course, was impossible. The result was a series of eddies, which must cause a certain amount of loss. He would ask whether, in laying down a certain number of pipes for conveying air or any other fluid, the Authors would choose a perfectly right-angled bend where the pipes had to make a change in direction, or whether they would choose a curved bend. For that reason Mr. Schönheyder considered that all fans should be made quite differently from that shown by the Authors. He had seen a very excellent work of the kind in some centrifugal pumps, made he believed by Messrs. Rennie, where a fan was so shaped that the water was given a gradual change of direction, and not a sudden change as shown in the drawings.

Mr. Bernays. Mr. J. Bernays observed that his experience had been more in connection with centrifugal pumps than with fans, but their action was so very much alike that he might say a few words on the subject. He had found the Authors' experiments very interesting; in fact, they had given the relation existing between the actual work done by a fan and the amount of work done by the air carried through it. At least, the experiments had been carried out with that view; whether they would give all that was wanted he was not quite so sure, because, like other speakers, he had been rather puzzled, first about the curve of efficiency and then about the formula given in the Paper. The efficiency curve seemed to rise from zero and to go back to zero. That could only arise from their taking at one end say the velocity and multiplying it by 0—that was, ignoring the pressure—and at the other end taking the pressure and not accounting for the velocity; in other words, they used the ordinary theoretical way of calculating the foot-lbs., but that did not in any way give the efficiency of the apparatus. It applied to all apparatus used for propelling air or water, or any other fluid; it therefore could not definitely or without modification be used for calculating the power required for driving fans. With regard to the form of fan-blades, he quite agreed with the Authors as to the efficiency of No. 4. He had been employed, more than thirty years ago, so far as centrifugal-pumps were concerned, in making experiments upon precisely the same point. It was true that they were only made with models, which, however, had been very carefully constructed; and he found as a matter of fact, that with a fan blade shaped as shown in the diagram, but still more distinctly bent in the same direction, the efficiency was very much greater so far as reduction of speed and the amount of work

Mr. Bernays.

done were concerned. Working with models it was not possible to go into the question of the power used. It had been found—and in that he agreed with Mr. Parsons—that with the best shape of blade shown it was absolutely necessary to have the cross-section of the fan not square but tapering, so as to keep the areas through the fan as nearly as possible alike. In the fan shown it could be readily seen that the air, after passing through the inner opening must considerably change in speed before it arrived at the outer circumference of the disk, unless a stream of air kept entirely to the forward surface of one blade and entirely left the back surface, thereby causing whirlpools and other inconvenience in working. There was one matter respecting which no practical data had ever yet been obtained to enable engineers to get at the real power used in centrifugal pumps and fans, viz., as to the pressures that existed both on the forward face and on the backward face of each blade whilst at work. He thought it was not absolutely impossible to obtain such data, and he recommended it to gentlemen who were in a position to do so, to try whether they could not make experiments and ascertain the exact pressure on each blade, step by step, from the tip to the inner circumference of the fan blade; they would then be in a position actually to calculate what was the total resistance, because a fan blade was in a position of the piston of a cylinder; it had pressure in front, and it had another pressure behind, the difference of those pressures producing the resistance for overcoming which the power was required. He did not know whether the Authors had considered what went on in a fan while the air passed through it. He had made a few calculations based on the figures supplied by the Authors to show the speed of the air through the fan. Taking the fan, Fig. 1, which had a disk of 28 inches diameter with openings 16 inches diameter, and a circumferential speed of 12,000 feet per minute, the delivery through the fan in one experiment was stated to be 8,000 cubic feet, as an average quantity. The fan at that speed would make 1,640 revolutions per minute, or 27 revolutions per second, equal to a discharge of 4·9 cubic feet (say 5 cubic feet) for every single revolution of the fan. As the cubical capacity of the fan-disk between the two annular flat sides was only 3·1 cubic feet, it followed that the time which a particle of air took to pass through the fan was not more than $\frac{1}{42}$ second, during which short space of time the whole action of the blades must be transferred into air propulsion. It was, therefore, of very great importance that fan blades should be of the right shape in order to give that movement without waste.

Mr. Lawn. Mr. J. G. LAWN desired to say a word or two on the point of no efficiency at a particular velocity of discharge. In the case of a mine, the air might be drawn through the fan, and might be sent through a pipe at such a speed as to show no water-gauge; but that did not mean that there was no efficiency in the ventilation, or that the ventilating machinery was entirely inefficient. In a mine, the water-gauge would not be put in the discharge chimney but at the inlet, at the top of the upcast shaft. He thought that was how the efficiency in that particular case could be calculated. If the reduction of pressure at the inlet of the fan were known, it would be possible to calculate the efficiency. As to the noiseless running of fans, it would be readily seen that, while the noise was troublesome in small fans, in large fans the shock became greater, resulting in vibrations and considerable injury to the fan. He would mention a device which had been successfully applied in large fans for overcoming that difficulty. Instead of having the upper edge (Fig. 12) which separated the fan chamber from the discharging chimney straight across, at right angles to the plane of the fan, if it was brought lower down and a V-shaped piece cut out of the partition above it, the point of the V being upwards, then the pressure on the fan-blades was not suddenly altered, producing vibrations, but was gradually altered, which in practice was found to quite remove the disagreeable and injurious shaking of the fan. The introduction of this device was due to Messrs. Waker Bros., of Wigan.

Mr. Terry. Mr. STEPHEN H. TERRY found that the Authors had shown the great importance of avoiding shock. Many of their experiments had that object in view, so that the blades should meet the air and also the air leave the fan with as little shock as possible, and in this connection he approved of the idea of fixed guide-blades, as it was obvious that any arrangement which tended to train the air in the desired direction towards the rotating blades would lessen the shock of impact when the air reached the blades. The reason for that arose from a fact that many engineers, architects and naval architects, were apt to overlook—namely, that air was, after all, a very heavy gas. Taking the barometer at 30 inches and the temperature at 60° F., 13 cubic feet of air weighed 1 lb. avoirdupois. In a room, for example, 60 feet by 40 feet and 25 feet high, there would be about 2 tons of air. He had had to do with a fan dealing with a volume of 300,000 cubic feet per minute, and with a weight of air of 600 tons per hour, a weight exceeding that dealt with by powerful dredgers. It was very clear that in dealing with a substance possessing such weight

as that, it was most important that the high velocity should be imparted to it first gradually and then rapidly; then the velocity having been imparted, the air should be allowed to lose it as gradually as possible, and it should while coming to rest, containing as it did (by virtue of its momentum) the power of doing further work, be made to do that work. In that connection he might mention that which was already known to those members of the Institution who read the Proceedings,[1] the invention of an Italian engineer, Saccardo, who had successfully introduced an arrangement which had for its object saving and utilizing the energy of the air as it left the fan. The relatively small volume of air, which was given a high velocity by passing it through a fan, was utilized in a species of modified injector, somewhat like a Giffard injector, to impart motion to a much larger volume at a lower speed. In that way a considerable portion of work stored up in the air in the form of momentum was taken; and he thought that any marked further economy to be looked for in fans would be found not only in designing fans so that the air which actually passed through them should be moved economically, but in using if possible the energy of the air when it left them, and making the comparatively small volume passing through the fan at high velocity impart a low velocity to a large volume of air. On such a principle the much-vexed question of the ventilation of tunnels could be more easily dealt with, and further economy than that at present obtained would be secured in ventilating mines. Possibly a modification of it might be used for ships, although that problem was rather difficult. Mr. Terry.

Mr. W. B. Crichton thought it difficult to understand why in Fig. 3 the efficiency should be zero at one end of the abscissa line and zero at the other. A fan was essentially a machine for passing large quantities of air at small pressures, and that being the case a very large proportion of the work must consist in overcoming the resistance of the atmosphere to displacement by the volume discharged. This was an argument in favour of not looking upon the lower efficiency curve as the correct measure of efficiency, but rather of taking the higher efficiency curve, *i.e.*, the curve calculated upon the compression and the water-gauge due to the velocity. By taking the efficiency in that way justice had scarcely been done to blade No. 2, because, referring to Fig. 6, it would be seen that the total efficiency curve remained practi- Mr. Crichton.

[1] Minutes of Proceedings Inst. C.E., vol. cxi. p. 454.

Mr. Crichton. cally parallel to the abscissas for the higher volumes, with blade No. 2, whereas with the other two blades, Figs. 7 and 8, the efficiency curves sloped down towards the end; and by the time a water-gauge of ½ inch to 1 inch was reached, the efficiency became very low; ½ inch to 1 inch was just about the maximum gauge at which the ordinary ship-fan worked in steamers fitted with cylindrical boilers, and for that reason the Authors had apparently overlooked the necessity of high efficiency at low gauges in the case of ship-fans. Another point was the output curve on Fig. 21. That curve was based upon the volume of the fan calculated by the diameter multiplied by the width, and he would like to ask the Authors whether they considered that if the width of any individual fan were doubled, the volume would be doubled. As far as his experience went there was a limit of width which, when reached, no increase of volume was possible. The volume must, to a large extent, depend upon the area of the inlet. There must be a considerable water-gauge—an internal water-gauge he might say—necessary to draw the air through the fan, or, in other words, to overcome the resistance of the fan itself, and if the inlets were made large that resistance must be less. The output curve at a tip speed of 5,000 feet per minute and about 1¼ inch of water-gauge showed an output of about 12 cubic feet per square inch of centre section. He had tested three weeks ago two ship-fans, 6 feet in diameter and 10 inches wide, and exactly at that pressure and tip speed they gave 22 cubic feet per square inch of centre section. Did the Authors consider that the very low output per square inch of centre section of their fans was due to the very great width of the fans in proportion to their diameter, and to the fact that the maximum useful width had been exceeded? Speaking generally, it would appear that blades of form No. 2, which gave a flat curve of total efficiency, were best adapted for fans which had to deal with varying quantities of air and low water-gauges.

Mr. Walker. Mr. W. G. Walker wished to put a question as to the method of blocking the output pipe. If he understood the Paper properly, the Authors had carried this out by means of plates having various-sized holes; but that was a very un-uniform way of baffling the output. The Authors had drawn attention to the importance of preventing shocks, and then the area of the output pipe had been reduced by means of placing plates directly across the pipe and at right-angles to the direction of the flow of air, which must have the effect of producing great shock. Mr. Walker had made some tests

within the last few weeks, baffling with a plate as the Authors had shown; but he found that with a plate of perforated zinc, a method employed by Mr. Donkin, he got much higher results. He had also used wire-gauze for baffling the output, and that gave higher results. The Authors' statement that "the air-discharge varies as the speed," was shown to be correct. He had also verified the statement that the brake HP. varied as the cube of the speed. Mr. Walker.

Mr. J. JEFFREYS had been engaged in experiments on centrifugal and propulsive fans for some time, and the results obtained were, in some respects, similar to those described by the Authors. He had also found that, when the air, entering a centrifugal fan, was deflected by fixed guide-blades in the direction of rotation, it passed more freely between the fan-blades, and obviated the eddying or churning effect produced when the direction of the air was parallel with the axis. With this addition, a distinct improvement had been found in the quantity of air discharged from the fan forming the subject of the experiment. The principle, he believed, was new as applied to fans, but had been previously adopted in turbines, and was described in Professor Unwin's lecture to the Institution a few years ago.[1] One cause of loss of mechanical efficiency in most centrifugal fans was internal skin friction at the periphery, where the air under compression was forced in a circular direction a mean distance of half the circumference before arriving at the outlet. In a fan designed by him, the air entered and was delivered parallel to the axis of the centrifugal blades, which were enclosed in a cylinder provided with fixed guide-blades, set at an angle of 45° round the internal circumference, against which the air from the rotating blades impinged, and was deflected in the direction of the outlet. The inlet was also provided with a short conical tube in which guide-blades, previously mentioned, were fixed, and the hub of the fan hollowed out to allow the air access to the back as well as the front of the revolving blades. The fan described was for exhaust ventilation, in which the quantity of air delivered was of greater importance than the velocity of its discharge, as in fans for blast purposes. The increase of the volume of air by heat, and the shrinkage of temperature following expansion had probably much to do with the observed variations of pressure in different parts of the outlet, Mr. Jeffreys.

[1] Inst. C.E., Lectures on Hydro-mechanics, p. 69, 1885.

Mr. Jeffreys. and suggested the advisability of avoiding constriction, and making the air-ways of uniform or slightly increased size from inlet to outlet.

Another source of loss was caused by bad workmanship. In a well-known fan the covers were of cast iron, roughly cored, and the revolving blades of ordinary angle-bars. With such a machine the skin friction and slip of air were unnecessarily great. In one of another series of experiments on propulsive fans without external covers, made with the view of determining the best form of blade, he had succeeded in obtaining a good delivery of air, though not quite parallel with the axis. He then fixed an enclosing cone over the fan with a small outlet at the apex. With a puff ball, suspended by a silken thread, he endeavoured to measure the angle of deflection, but was astonished to find that the puff ball promptly entered the hole no matter how fast the fan was turned. This was caused by the air impinging against the periphery of the cone, setting up an eddying or rotating motion and forming a partial vacuum in the interior. It was possible for a propulsive fan fixed in an air-shaft to rotate without doing any useful work, owing to high frictional resistance. He knew of a case in which superior results were obtained by removing the fan and allowing the air to flow through naturally.

Mr. Platt. Mr. J. Platt asked the Authors to make a slight correction in the location of the Parkend Colliery, which was not in South Wales but in the Forest of Dean in Gloucestershire. Things were very bad there; the iron trade had left the district; there were no furnaces in blast, but he had known seven in blast at one time. A few collieries, however, were left, and it was only fair to make it known that at least one was at work. He was much obliged to the Authors for the pains they had taken. The subject was a very old one. He remembered as a boy visiting the Patricroft Works, where the late James Nasmyth showed him a fan with which he had been experimenting for colliery ventilation. It was, he believed, the first direct-driven fan. There was a small vertical engine direct on the spindle. Nasmyth was trying it with an enclosed case with vanes open to the atmosphere, to ascertain the various effects. He did not know whether the experiments were published. It was, he believed, the first case of a ventilating fan put to a colliery.

The Authors. The Authors, in reply to the discussion, thought that a little reflection would probably satisfy objectors that the efficiency

curves should dip down as shown on the various diagrams. Comparing the case with that of a centrifugal pump, each characteristic curve showed what would happen supposing the pump to rotate at a constant speed, and the lift to be gradually reduced. Increasing quantities of water would be delivered, and the point where the efficiency curve touched the zero line would correspond with the pump working against no lift, but merely passing water from one reservoir into another at the same level. The term "efficiency" seemed scarcely to apply to this process. A little distinction needed to be drawn between an exhaust-fan and a pressure one. The Authors reckoned the efficiency of an exhaust-fan on the vacuum it could maintain on any given reservoir, and similarly the efficiency of a pressure fan on the compression it could maintain in any vessel or pipe. The air-velocity was only really useful in so far as, from the design of the fan or other causes, it helped to increase the vacuum or compression referred to. Mr. Parsons recommended graduating the inlet of a fan. The Authors, however, did not believe it to be of material advantage, and this apparently was also the experience of Mr. Bryan Donkin. Of the eleven fans tested by that speaker, a fan with six straight blades and perfectly plain inlets gave the highest mechanical efficiency. Perhaps the case of air flowing into the inlet of a fan might be compared with that of water flowing from a sharp-edged circular orifice of a vessel, the suction of the fan-centre corresponding with the action of gravity on the water after it had passed the orifice, the air taking a natural curve just as the water would do. Bell-mouthing the orifice would cause it to pass more water (or air), but the frictional loss would be about the same; and an equally simple and cheaper method of passing more air was to slightly increase the diameter of the inlet, and of course the diameter of the fan. A suitable bell-mouthed inlet might be of advantage in very narrow fans. The Authors had read the results of the tests of Mr. Bryan Donkin with great interest. Although their Paper only dealt with one type of fan, careful experiments had been made with practically every description of fan obtainable, and on a large number of different forms of vanes not mentioned in the Paper. Several of the fans were rotated the reverse way in their casings, for instance, blade No. 2, Fig. 5. The difference was not great, for in this vane the output was somewhat increased, and the mechanical efficiency diminished. The Authors had purposely refrained from instituting comparisons between fans made by The Authors.

The Authors. different manufacturers. The Heenan fan, on which all the tests in the Paper had been made, was the result of a somewhat exhaustive series of tests extending over several years, which tests embraced, as far as the Authors could see, all the important points mentioned in the discussion. The journals of the Heenan fans tested were of hard steel, running in cast-iron swivel bearings, which had been found to give a very satisfactory result. The anemometers used in the tests referred to were calibrated at intervals, at Kew Observatory, and that department had supplied a table of the errors of the instruments for various speeds. The errors were small. With regard to the number of vanes, the Authors used six for anything below 8 feet in diameter. For fans above that size the number was increased. In reply to Mr. C. H. Wingfield, the efficiency of a fan discharging into the stoke-hold of a torpedo-boat would be determined by measuring the difference of compression between the stoke-hold and that portion of the vessel where the suction of the fan was placed. The remarks made by Mr. Wingfield on two fans running in parallel were interesting and in accordance with the experience of the Authors. With regard to the side-gauge tips, a pipe placed flush with the inner edge of the tube would measure correctly the compression existing at that point, but the value might be different at the centre of the tube if the air had any considerable velocity. Hence the advantage of a movable side-gauge. In reference to the remarks of Mr. W. Schönheyder on the shape of the vanes; if a vane had a radial tip, the velocity of the air on leaving the periphery of the fan would be very nearly the same as that of the periphery, whatever the length of the blade. The length of the vane might be designed to give the air a uniform increase of whirling velocity during its passage through the fan-centre, and this was the condition used by the Authors to fix the length of some of the blades. Too much importance, however, should not be attached to this requirement. The Authors believed that if Mr. W. Schönheyder had further experimental knowledge of fans, his views on the advantages of curved inlets would be considerably modified. Reasoning on experience gained with centrifugal pumps, Mr. J. Bernays had attached great importance to the use of tapering side-plates on the revolving centre. The Authors had not found them of any advantage on fans, and indeed they did not see much advantage in preserving a uniform cross-sectional area when the air-velocities at different points in the same cross-section were pretty certain to be widely different. Measurements

of pressures taken near the revolving vanes would doubtless be interesting, but probably not of much practical advantage; and generally speaking, if mathematical investigation was applied to this subject, the Authors considered it should be at sections well removed from the revolving portion. The Authors.

Correspondence.

Professor Boulvin, of Ghent, remarked that the Authors' experiments, although carried out with apparatus belonging to a particular system, gave results which were applicable to all reaction fans. From comparison of Figs. 13 to 16, Plate 5, it would seem that the curves showing the pressure at different speeds lacked continuity at speeds of 6,000 feet and 8,000 feet per minute; at least it was so when, taking as ordinates the volume and the pressure which gave the best output at these different speeds, the circumferential speeds were plotted as abscissas. There would probably be some reservations to make on the measurement of the delivery by means of an anemometer when it was applied to a volume issuing at high speed from a circular section of small diameter. It was not stated by the Authors how they had taken account of the contraction of the stream; and the anemometer itself produced an obstruction which modified the conditions of speed, even when the section was very great. The anemometer also did not permit of the exploration of the section to the edges. How far the measurement of the delivery could be exactly arrived at by means of that instrument, which he supposed carefully calibrated, was a question which the Authors ought to have decided experimentally. In the evaluation of the mechanical efficiency, the only one considered by the Authors, two figures, derived from two curves, in full and dotted lines, were given. In the first hypothesis account was only taken of the pressure of the air driven back; in the other, to that pressure was added that necessary to raise the speed to the point at which it was measured. In the case of the 28-inch fan, efficiencies of 73 per cent. and 80·5 per cent. were obtained respectively. The brake horse-power, of which account was taken, included the friction of the axis of a rope-pulley and of the axis of the fan, as well as the resistances of a belt; so that the figures ought to be considered extremely high, and were probably higher than all the results obtained hitherto. But it did not seem to him proper to add to the height due to compression Prof. Boulvin.

Prof. Boulvin. that due to the speed; the energy conserved by the air in the form of speed was a defect of the fan and constituted a loss, for it was necessary to cover in fans with expanding chimneys. It did not seem permissible to accept the figure 80·5 per cent., which would require the fan having an expanding chimney to possess an efficiency equal to unity. He considered *Fig. 2* gave a false impression; a machine which would raise water in discharging at a high speed ought not to be credited with the energy which was also lost in the same degree as that expended in wire-drawing through valves, etc. There were in reality very few cases in which a jet of air driven at high speed was demanded of a fan. He believed the facing gauge, which measured the velocity of air as the Pitot tube measured the velocity of water, and had frequently been used, notably by Mr. Bryan Donkin, in analogous experiments, was an excellent instrument when the heights of the columns were read with precision, and it could be placed close to the sides of the conduit. In supposing the extremity of the tube struck by a current of air at the velocity v, the Authors wrote, "The pressure h, in feet of air, due to a velocity of v feet per second is, $h = \frac{v^2}{2g}$." He did not contest this result, which experience had established as exact; but the proof was subject to a slight correction. Without doubt the air would flow away under the pressure h, with the speed v given by the formula; and it was equally true that if the air penetrated, driven at the speed v, into the small end of a widely expanding tube, on arriving at the enlarged section it would have regained the difference of pressure measured by h; but the case of a bent tube was quite different. The air, driven at velocity v, struck a column of air, which was quite immovable in the tube. It was also easy to demonstrate that if the air driven at velocity v did not after the shock conserve any component of the original speed, the height h measuring the pressure produced in the curved tube would be $\frac{v^2}{g}$ instead of $\frac{v^2}{2g}$; but the air deviated by the shock conserved a certain quantity of motion by lateral flow; so that in reality $h = k\frac{v^2}{g}$, k being a coefficient smaller than unity, depending probably in certain measure upon the section of the orifice and of that of the conduit in which it was situated. When the orifice was small and the conduit great, experiment showed that $k = \frac{1}{2}$, confirming the Authors' formula, of which the resemblance

with the well-known hydraulic equation was a pure analogy and could not be called as a demonstration. It was well known also that when the formula was applied to find the pressure of the stream on a surface normal to its direction, the coefficient k depended on the amount of that surface and on the sharpness or roundness of its edges. Prof. Boulvin.

Mr. ARTHUR R. BROWN was glad to find that the results of the Authors' experiments confirmed those of trials he had undertaken at the request of Mr. Andrew Laing, of the Fairfield Shipbuilding and Engineering Company, at their works in 1886; the object of which had been to find a better form of blade for fans for forced draught required for some fast paddle-steamers. Owing to a peculiar construction of the passages between the fan and the furnaces, it had been found that a fan with the ordinary form of blade would not deliver the air at a sufficiently high pressure at the furnace. The fans experimented upon were all 4 feet in diameter, 12⅝ inches wide, having double inlets, each 19¼ inches in diameter; and each had six blades, the different forms of which were exactly similar to the three shown in *Fig. 5*. The fans had all been tried in the same casing, similar to that shown in *Fig. 1*, and had been driven at as nearly as possible the same speed. The discharge-pipe measured 21 inches by 21 inches; and its area was varied by means of a sliding shutter placed at an angle of 60°, readings being taken when full, three-quarters, half, and one-quarter open. The experiments had proved that No. 3 blade, as shown in *Figs. 5*, was a decided improvement, in all cases as regards pressure and volume, on No. 2, which was considered to be the ordinary type of blade then in use, and No. 3 blade had been, at the time these experiments had been made, thought to be a new invention. The fan had been found to be improved by straightening and bringing the tip forward; and it was, therefore, thought that if the tip was made radial and curved, as shown in No. 4 blade, *Fig. 5*, still better results might be expected. This had been found to be the case, both as regards pressure and volume of air discharged for the different speeds and for the various discharge openings tried, No. 4 blade giving a water-gauge of 13 inches at the highest speeds on a reduced opening, against a maximum of about 9 inches given by No. 3 with the same area of discharge. Consequently, the curved radial tip form had been adopted, and drawings sent to the fan-makers with instructions to make all fans for the Fairfield Company in that manner. A few different fans had then been Mr. Brown.

Mr. Brown. tried to get the best proportion of diameter, width, and diameter of inlet. These experiments could not be carried out to any length; but, as far as they had gone, the results showed that the diameter of the fan should be 3·8 times the width, and the diameter of the inlets each one-half greater than the width. Single-inlet fans, with the same form of blades, had been tried but had not been found to be equal to those with double inlet. The horse-power required to drive the fans had not been considered an important point, as when a fan was required to give a certain pressure at the furnaces for engines of, say, 4,000 HP., 1 HP., or a fraction of 1 HP., more or less required for driving the fan was not worth considering so long as the fan gave the pressure required, it having been found that the ordinary fans and also No. 3 form gave no increase of pressure for increase of speed beyond a certain point, which could not be found for the curved radial tip form owing to the engine being unable to drive it at a sufficiently high speed. In January, 1894, he had stated to the Authors the results of these experiments, and had sent them sketches of the three forms of blades tried. He had pointed out also that the essential point was to make the portion of the blade where it joined the circumference radial, and that the blade should present to the circumference of the inlet an inclination following the resultant of its movement of rotation, and the movement of the air penetrating the spaces between the blades; from this it should incline by a gentle curve, and terminate by a part directly following the radius. A form of blade based on these principles was shown and the theory fully worked out in Mr. D. Murgue's book on centrifugal ventilators. He had pointed out also to the Authors that probably a form of blade based on these principles would be found to give even better results than the curved radial tip, and had asked Messrs. Heenan and Froude, when they made their intended experiments, to try this form together with the three other types. He thought it a pity this had not been done, as the experiments would have been more complete, and the theory of centrifugal ventilators as given by Mr. Murgue would have been put to a practical test. He also thought it would be found advisable to make the blades as numerous as possible, so long as they did not choke the inlet. In Mr. Bryan Donkin's experiments,[1] a fan after this style with a large number of blades (Figs. 1 and 4, Plate 2) had been found superior to the

[1] Minutes of Proceedings Inst. C.E., vol. cxxii. p. 265.

other forms tried, and would probably be found to be better than the curved radial tip; although, perhaps, for small fans required to be used on board ship, the extra cost and complication incurred when so many blades were used might not compensate for the extra efficiency. The Authors were to be congratulated on the careful manner in which the experiments were conducted, and the methods employed to get the most accurate results, especially as regards their method of testing the accuracy of the water-gauge tips. Mr. Brown.

Mr. D. MURGUE, of St. Etienne, observed that the results stated in the Paper had been obtained by irreproachable methods, and accorded with those which had been found in France. They were of a kind to effectually guide engineers of collieries in their installations of ventilating plant. The Authors' experiments had, however, been limited to three forms of blades, having inclinations at the outer circumference of 35°, 60°, and 90°, the last giving the most favourable results. He considered that if they had been extended to blades with forward inclinations of 120° and 135° still better results would have been attained. In France the centrifugal fans most used, such as those of Messrs. Ser, Rateau, and Mortier, all had a forward inclination of the blades at the extremities of as much as 135°. Numerous experiments had placed the superiority of such an arrangement, which besides conformed to the indications of theory, beyond doubt. Mr. Murgue.

Mr. M. PAUL, of Dumbarton, regarded the division of the air-pressure into "compression" and "velocity" pressure as a novel feature of great value in the analysis of the results. The rise of the compression-curve in Figs. 8 and 20, Plate 5, after the opening of the discharge orifice, seemed difficult to explain. In all the other diagrams there was a gradual fall in this curve, as was to be expected. Although the differentiation of the total pressure was of theoretical value, it might be questioned whether, in view of the fact that the ordinary requirement for a fan was the delivery of a certain volume of air at a certain pressure, results of greater practical value would not have been obtained by allowing the fan to discharge into a large chamber, instead of through a small pipe, thus eliminating the errors due to eddies, etc. This exhibited at once what might be called the "commercial efficiency," and was the method adopted in most of the trials at the Levenford Works, Dumbarton; and, although the absolute results might not be so reliable as those given in the Paper, the comparative results of those trials accorded in every respect with the deductions made by the Authors. To the three formulas given at foot of page 279, which were confirmed by his results, and had been used at Levenford Mr. Paul.

Mr. Paul. Works for some years, the following were useful additions, being deduced from these three, but confirmed experimentally.

$$\text{Volume} \times \sqrt{\text{gauge-reading}} = \text{a constant, for constant speed.}$$
$$\text{Volume} \times \text{gauge-reading varies as speed}^3,$$

or,

$$\text{Volume} \times \text{gauge-reading varies as the brake horse-power.}$$

It would be found that these formulas were also confirmed by the results plotted in the Paper. With them, having one curve for each fan at varying speed, it was simple to find the volume corresponding to any pressure, or *vice versa*.

The relative size of inlet, reference to which had been omitted from the Paper, was a very important factor in the performance, and reliable comparisons could only be made when it was taken into account. It appeared from the "centre-section" areas of the 18½-inch, 28-inch and 48-inch fans, that they were not quite similar, the widths being respectively 0·5, 0·465 and 0·31 of the diameter. The curves, Figs. 18, 19 and 20, could therefore only give an approximate comparison of the output of these fans. From theoretical considerations it appeared that the duty of two similar fans driven at the same speed varied as the fifth power of the diameter; and this again, within reasonable limits, was borne out by his results. This formula was of great use in interpolating curves for sizes of fans for which no experimental results had been obtained. It had been suggested to him by Mr. John Ramsbottom, that the best means of determining the duty of a fan would be to direct the discharge into an ordinary gas-holder, thus securing a constant pressure, and eliminating all errors of in anemometrical readings. This would certainly be an ideal method of testing, but the dimensions of the apparatus required formed an obstacle to its adoption.

Prof. Rateau. Professor A. RATEAU, of St. Etienne, could accord, in some of the important questions raised in the Paper, with the conclusions of the Authors, but considered that others of them should be received with reserve. As the Authors had said, it was very difficult to conduct experiments on fans in a manner to give sufficient exactitude. The measurement of the compression or depression of the air and that of the quantity of air delivered per unit of time gave results widely differing among themselves according to the manner in which they were carried out, and care was necessary to compare among themselves figures given by the different experimenters. The compression of the air had, in the Authors'

experiments, been measured in the full current of air and close to the fan by the aid of a straight tube furnished with a perpendicular plate, to give the static pressure, and of a bent tube, or Pitot tube, which gave the total pressure, the static pressure plus the dynamic pressure corresponding to the speed of the current. In the immediate neighbourhood of the fan these pressures varied widely among themselves according to the point of section of the current at which they were measured. That could be remedied by taking, as the Authors had, the average of many observations over the whole cross-section. The measurement of the deliveries, however, presented a greater objection. It had long been known, and the experiments of the Prussian Commission at Breslau had confirmed, that anemometers indicated the speeds too high by between 10 per cent. and 15 per cent. The delivery of the air had also been measured by the Authors in a section presenting an abrupt contraction of the tube. In that case the contraction of the filaments of air caused an irregularity in the record of the anemometer, resulting in a still further exaggeration of the delivery. He was led to these conclusions by the results of many experiments he had conducted on fans. Prof. Rateau.

Of the different methods he had tried the best seemed the following: The fan was arranged to discharge into a large box from which the air escaped by a convergent conical tube. In this box the air had a sufficiently small speed that the dynamic pressure was negligible; there was then no uncertainty as to the compression given by the fan and measured in the box. From that compression the delivery could be deduced by multiplying the final section of the convergent tube by the speed corresponding to the compression; and there could be but little error on the result, for it was known, as had been shown by Mr. Hirn,[1] that a conical tube gave a flow at the full section within 2 per cent. or 3 per cent. He believed, therefore, that measurements of the delivery by the anemometer, such as were made in mines and such as the Authors had made, were too high. From this exaggeration of the delivery resulted, as a concomitant consequence, a similar exaggeration of the mechanical efficiency. In the present case the mechanical efficiencies were not in accord with the manometric efficiencies. For blades terminating in a radial element these two efficiencies should be equal or little different, as he would show by a formula, whereas the curves shown in Plate 5 showed at the normal course, for example, 0·70 for the mechanical

[1] Annales de Chimie et de Physique, vol. vii., 1886, p. 289.

Prof. Rateau. efficiency and 0·56 for the manometric. The method employed by Mr. Bryan Donkin[1] seemed better than that of the Authors in that it avoided the use of the anemometer for the measurement of the delivery. The method he had described was similar, but was preferable because it required but a single manometric reading which gave at once the compression and the delivery. The "ouverture" on which the fan was working could be varied by changing the orifice of the convergent conical tube.

With regard to the best number of blades to adopt for each type of fan, his experiments showed that it depended greatly upon the kind of apparatus employed. For a centrifugal fan designed for relatively small deliveries of air between six and eight blades were sufficient. But for apparatus delivering large quantities, in the interior of which there were relatively high speeds of circulation, it was necessary to have twelve, eighteen or sometimes even more blades. It could be taken as an approximate rule that the total surface of the blades ought to be proportional to the volume delivered for unit of time divided by the peripheral speed of the fan.

A theoretical formula for fans had for a long time been established. He had generalised and simplified the formula to the following[2]:—

$$g\,H = V_1\,a_1 - V_o\,a_o$$

where H was the pressure furnished by the fan, estimated in column of the fluid circulating in the apparatus, V_1 the speed of the blade where the air leaves it, a_1 the projection on that speed of the blade of the absolute speed of the air where it leaves the blades, and V_o and a_o the analogous elements to V_1 and a_1 at the point of entrance. V_1 and V_o were necessarily positive; while a_1 and a_o could be either positive or negative. This formula could be applied to all kinds of fans as well as to centrifugal pumps. The pressure H, in inches of water h, could be found by introducing the specific weight of the air at the particular temperature and pressure, 0·0012 nearly.

$$h = 0{\cdot}0012\,H.$$

For centrifugal fans such as that of Mr. Heenan, the air entered nearly radially into the moving wheel, then a_o is zero, and

$$h = \frac{0{\cdot}0012}{g}\,V_1\,a_1\,;$$

[1] Minutes of Proceedings Inst. C.E., vol. cxxii. p. 265.

[2] See also "Considérations sur les Turbo-machines et particulierement sur les ventilateurs," by A. Rateau, Bulletin de la Société de l'Industrie Minérale, 1892.

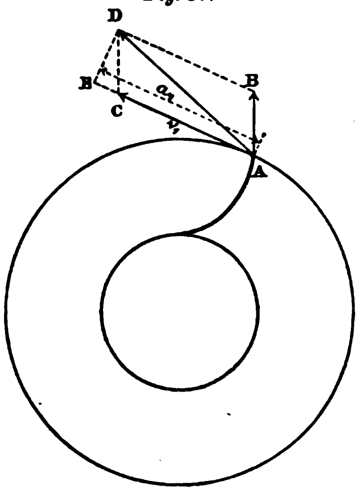
Prof. Rateau.

a_1 was the projection of the absolute speed A D, *Fig. 30*, on the peripheral speed of the wheel A C, that was A E; a_1 would be exactly equal to V_1 if A B was perpendicular to A C, that was to say, if the last elements of the blade were radial. The equation would become then

$$h_2 = \frac{0 \cdot 0012}{g} V^2,$$

V being the peripheral speed of the blades. But a_1 was smaller or greater than V_1 according as the blade was curved at the periphery backward or forward. This would show why, in the experiments of the Authors, an improvement of the compression had been indicated (at the same peripheral speed) when the blades were straightened towards the radius. This improvement would have continued if the blades had been turned back towards the front. It did not necessarily correspond to an improvement in the mechanical efficiency for the causes of loss in the interior of the fan could vary with the form of the blades. The formula he had given gave the key to the different properties of fans. But it required for practice a coefficient of reduction. This coefficient ought to take into consideration the losses in the interior of the wheel, as also of the diffuser for transforming into pressure the *vis viva* of the fluid leaving the wheel. It should be remarked that the diminution on the pressure or compression occasioned by those losses in the interior of the fan would give nearly the loss of mechanical efficiency. This ought to be smaller than the manometric efficiency on account of the waste work independent of the air itself.

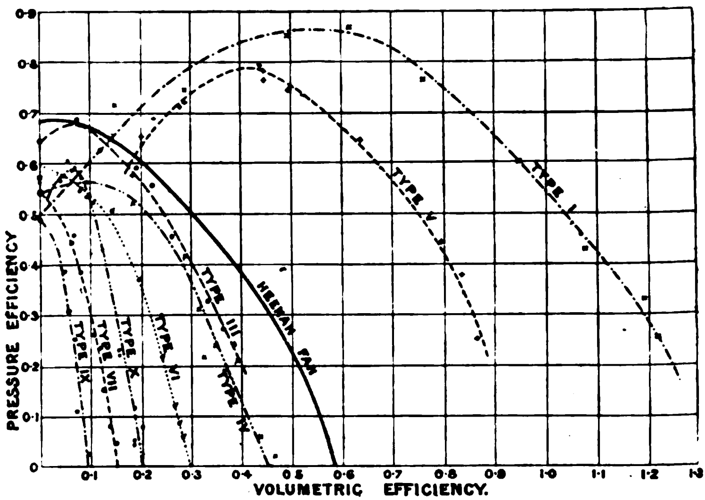
Fig. 30.



In the Authors' characteristic curves, the deliveries of air had been taken as abscissas, while on the Continent the equivalent orifices are the relations between the delivery and the square root of the compression. It seemed preferable to plot the delivery, or better the coefficient he was about to define. The analogy between these curves and those of a dynamo-electric machine he had pointed out in the work he had referred to. The curves had a

Prof. Rateau. simple shape resembling much a fragment of parabola, as was shown in Fig. 1, Plate 5. It was now necessary to define the "coefficient of delivery" and the "manometric power." It had been remarked by the Authors that the characteristic curve would be rendered independent of the size of the fan by taking the ratio of the delivery to the section of the opening of the entrance of the fan. But they traced the curves for each peripheral speed. He had shown in the Paper referred to, that the curves could be also rendered independent of the speed by dividing the delivery by the speed. With some exceptions, then, a type of fan was represented by a single curve. The "manometric power" was

Fig. 31.



the ratio between the pressure experimentally determined each time to that which was shown by the formula $h = \frac{\pi}{g} v^2$, π being the specific gravity of the air. The "coefficient of delivery" was given by the formula $\delta = \frac{Q}{r^2 V}$, where Q was the experimental delivery, r was the radius of the moving wheel, and V the peripheral speed. The delivery should be brought back to a single opening. For a fan with two openings the formula would be $\delta = \frac{Q}{2 r^2 V}$. The manometric power should not be confounded with the manometric efficiency, which was given by the complete formula giving the theoretical compression, $g\,H = V_1\,a_1 - V_o\,a_o$.

The power and the efficiency were not equal except for fans with radial blades or those in which $V_1 a_1 - V_o a_o = V_1^2$. It should be remarked that the manometric power and the coefficient of delivery were simple numerical coefficients, independent of units of length, of time and of mass. If the coefficient of delivery were plotted as abscissas, and the manometric power as ordinates, curves were obtained which corresponded with those of Mr. Bryan Donkin for different types of fans, *Fig. 31.* On this has been designed the characteristic curve of the Heenan fan, as shown by the curves given by the Authors. The great height of the curves indicated a great efficiency to produce a compression, and a delivery power was shown by the great amplitude in the direction of the abscissas. It was remarkable that the curves were nearly parabolas with axes parallel to the ordinates. A type of fan could therefore be determined by three numbers, the coordinates of the summit of the parabola and its parameter, or three other equivalents. Prof. Rateau.

Mr. A. L. Steavenson had, in 1866, brought under the notice of the North of England Institute of Mining Engineers,[1] the ventilator of Guibal, who had then adopted the same shape of vanes or blades[2] as those adopted by the Authors. Mr. Steavenson had since tested carefully fans of sizes varying between 10 feet and 50 feet in diameter, when working on mines, and had found that tests made on small ventilators, such as that described at p. 273, *ante*, did not afford any guide to the efficiency of a large fan, as such small influences served to vitiate the results. The water-gauge reading, for example, given at the inlet of a fan was no guide to the vacuum which would be induced in the mine. A small fan would show a high vacuum at the inlet, but at a few yards distance the water-gauge reading would be perhaps 50 per cent. less, the fan being too small to swallow the volume of air, of which the mine and the shafts would pass at the vacuum given at the inlet, and which was therefore a false vacuum. He therefore considered that a large ventilator was necessary to get a large volume efficiently. When testing ventilators he had always placed the water-gauge sufficiently far from the machine to ascertain a vacuum which is really generated in the mine. In the measurement of the air erroneous results were often obtained. He had found, in the tests of a fan which was said to be giving over 70 per cent. of useful effect, a result derived from anemometer readings in the centre of the drift, that much of the air which was going towards Mr. Steavenson.

[1] Transactions, North of England Institute of Mining Engineers, vol. xvi. p. 11.

[2] See also "Theories and Practice of Centrifugal Ventilating Machines," by Mr. D. Murgue. E. & F. N. Spon, p. 18.

Mr. Steavenson. the fan above the centre of the drift, was reversed and returned near the floor. After dividing the drift into small areas, and measuring the velocity in each, he had obtained a useful effect of between 40 per cent. and 50 per cent. Such machines, however, were very costly, some of those in use in the North of England, with their boilers and engines, having cost £6,000. It had therefore to be determined whether it was not better to instal a less efficient fan at a smaller cost.

The Authors. The AUTHORS, in replying to the correspondence, stated they did not make any allowance for a coefficient of contraction at the orifice where the volume of air was measured by the anemometer, nor did they think it necessary to do so. In reference to Figs. 9, Plate 5, it should be stated that three orifices were arranged for use at the outlet, having areas of 1½ square foot, 3 square feet, and 7 square feet respectively. The largest outlet, having a greater diameter than the boiler flue, was connected with it by means of a bell-mouthed conduit and a uniform discharge was secured over the whole area of the outlet by the insertion of small guide-vanes in the conduit. Now it was certain there was no contraction to be allowed for in the case of the large outlet, and, as each of the three outlets were used in succession for measuring different deliveries of the same fan, the air-discharge curves obtained from these results would not be continuous if any contraction of the air-stream took place when using the smaller orifices. But those curves were continuous, within very small limits for the three orifices, and, therefore, the Authors regarded their measurements as correct. It might be remarked that the air was not under compression in the half of the boiler flue nearer the outlet, but was more in the condition of a jet of air flowing parallel to the axis of the flue, and impinging normally on the end where the outlet was fixed. Hence it was difficult to see how any contraction of the section could be set up at the orifice. In the case of the 28-inch fan referred to by Professor Boulvin, the efficiency, 85 per cent., shown by the dotted lines on the characteristic curves, Fig. 8, was reckoned on the total gauge, and should, of course, be only credited to the fan if the conditions were such that the whole of the velocity-area of the air was utilized. If the fan were fitted with a perfect expanding chimney the efficiency would then be 85 per cent., and could fairly be credited to the fan. The efficiency would not be equal to unity. As pointed out by Professor Boulvin, the equation $h = \frac{v^2}{2g}$ did not necessarily hold unless the air could lose velocity and gain pressure-

energy without shock. It was because the Authors were not satisfied on this matter that they undertook the experiments on the gauge-tips described in the Paper. The Authors.

In reply to Mr. Arthur P. Brown, it might be remarked that, so far as shape of blade was concerned, the theory of centrifugal ventilators as put forward by Mr. D. Murgue would be the same for any given form of blade, provided the inlet- and outlet-angles were the same; hence, the reasoning given in that treatise was applicable to the Authors' results. They did not find any difficulty, as suggested by Professor Rateau, in taking the necessary measurements of pressure in the immediate neighbourhood of the fan. It was quite true the velocity of the air varied widely at the cross-section in question, but the compression was very uniform, and was generally about 2 per cent. or 3 per cent. in excess of the compression in the adjacent portion of the boiler flue. The measurement of the air-velocity at that section was only required as a rough check on the anemometer readings, and was not always observed. The formula given by Professor Rateau to determine the water-gauge developed by a fan for any particular tip-speed was new to the Authors and simpler than the usual expression. But all such formulas rested upon assumptions of very doubtful accuracy, and the Authors preferred to proceed in the manner indicated in the Paper. The method of plotting the characteristic curves recommended by Professor Rateau practically differed from that of the Authors' in that he divided all the air-discharges, taken with any particular resistance plate, by the speed, and the pressures by the square of the speed. A characteristic curve was thus obtained which did duty for all speeds, but from which absolute results could only be obtained on multiplying by suitable constants. The Authors believed their curves preferable in that they gave directly quantities that were actually required in practice, and, in addition, they furnished a clearer view of the performance of any fan. They could also take account of any variation of the efficiency with the speed. They were, however, prepared to admit the saving of labour in plotting results effected by the method recommended by Professor Rateau. As mentioned by Mr. A. L. Steavenson, a test of a small fan did not necessarily afford any guide as to the mechanical efficiency of a machine of much larger size, although the Authors found the output of each fan followed the same law very nearly. When testing a mine-fan, the Authors usually measured the air-discharge at the top of the expanding chimney, in the manner mentioned in the Paper.

"Note on the Movement of the Walls of the Kidderpur Docks."

By James Henry Apjohn, M.A., M. Inst. C.E.

Reply of Mr. Apjohn to Correspondence.[1]

Mr. J. H. Apjohn, in reply to the Correspondence, observed that the dock-walls had been founded by excavating in open cutting to a depth of 27 feet below the surface, and sinking timbered trenches 17½ feet in width, or half the thickness of the walls, for the remaining 19 feet of the depth. It had been found necessary to insert the brickwork as soon as a small portion of one trench had been bottomed, as otherwise the ground rose in the trench. He thought the driving of piles in such a trench would have been accompanied by such vibration as to cause a universal upheaval of the site of the walls. About twenty-two years ago an attempt had been made to pile the foundations of a lock on the Hijili Canal in soil similar to that encountered at the Kidderpur Docks. The area of the lock foundations had been excavated with easy slopes to foundation level, only 25 feet below that of the ground, and pile-driving had been commenced, with the result that the banks had sunk all round the excavation and the whole bottom had risen about 10 feet, spewing up the piles which had been driven. Even if the level of the dock-wall foundations had been raised 5 feet, the depth of excavation, if the piles had been driven in open cutting, would have exceeded 40 feet; and experience of the soil proved that the slopes would have had to be made as flat as 7½ to 1. Such an open cutting had never been excavated in the alluvial soil of the Delta of the Ganges, and it was quite uncertain what the result of making it would be. The alluvial strata had never been penetrated, though borings had been carried at Calcutta to a depth of about 350 feet. There was no reason to suppose that the soil improved at greater depths; but, on the contrary, there was evidence that at about 60 feet below the surface there existed a layer of water-bearing sand, the water being under

[1] Minutes of Proceedings Inst. C.E., vol. cxxi. p. 143.

pressure, and when tapped rising to ground-level. He thought it possible that if two-thirds of the soil above this sand were removed, the pressure of the water in it would upheave the remaining 20 feet of soil. The strata plotted from borings made on the site of the docks previous to their construction showed this sandbed in some cases dry; but all through the construction of the works a strong spring was experienced wherever a bore-hole had been made, the water from such springs being all that had to be pumped out except the rainfall. The drainage of the works during construction had been most complete. Borings recently made in connection with a project for a railway tunnel under the Hooghly, 15 miles below Calcutta, had disclosed the existence of a similar stratum of water-bearing sand at about the same depth as that at Kidderpur, and when tapped the water had risen above ground-level. The only mistake at Kidderpur had been the exposing of the wall to pressure from the backing, instead of counteracting that pressure by an efficient bank of earth against the face of the wall as high as the coping level, to be dredged out after the admission of the water. The walls were, however, now as good as if they had never moved, and the loss of time and money caused by their movement had been trifling compared to that which would have been involved by founding them on piles, even if such a course had been practicable.

SECT. II.—OTHER SELECTED PAPERS.

(*Paper No. 2778.*)

"The Efficiencies of Gas-Producers."

By CHARLES FREWEN JENKIN, B.A., Assoc. M. Inst. C.E.

IN a Paper[1] read before the Institution in 1886, Mr. F. J. Rowan gave a complete historical summary of the different forms of gas-producers. He states, however, that the investigation of the thermochemical problems connected with their efficiency is outside the scope of his Paper; and he does not attempt to compare the efficiencies of the producers described. The use of gaseous fuel is now so widely extended, that a reliable method of comparing the efficiencies of the different appliances for its production seems most necessary.

In this Paper the Author points out the terms in which the efficiency of a producer should be stated, how the efficiency may be calculated, and what data are required for the calculation. The calculations are founded on those employed by Professor Åkerman,[2] and Mr. von Jüptner, but are all made directly from the volumetric analyses of the gases, without the use of analyses by weight. For this purpose the Author has calculated Tables of the heat of combustion, specific heat, &c., referred to 1 cubic metre of gas instead of to 1 kilogram of gas. The methods of chemical analyses are selected from Dr. W. Hempel's "Gasanalytische Methoden."[3] Most of the examples required as illustrations are taken from the Author's experiments; a few, however, have been selected from those of Professor Åkerman and Mr. von Jüptner.

Producer-gas is used for many purposes, but in all cases the primary object is to supply heat; this heat is, of course, derived from the coal fed into the producer. The efficiency of the producer is therefore defined as the ratio of the heat contained

[1] Minutes of Proceedings Inst. C.E., vol. lxxxiv. p. 2.

[2] "Beitrag zur Entwicklung der Frage der Heizgasgewinnung," von Richard Åkerman. Auszugsweise übersetzt aus Jernkontorets Annaler 1891, von Hanns von Jüptner. "Berg- und Hüttenmännisches Jahrbuch," vol. xl. p. 81.

[3] An English translation, by Mr. L. M. Dennis, has been published; London, Macmillan & Co., 1892.

in the gas as it leaves the producer to that contained in the coal from which the gas was made. The heat contained in the gas may be divided into two parts, the heat of combustion, and the sensible heat due to its temperature. When the gas is cooled before it is burnt, the latter part of the heat is abstracted by the cooling arrangement and is no longer available. It will be useful to consider the efficiency of producers both when the gas is used hot, and when it is used cold. For simplicity the Author calls these two values, the hot-gas and the cold-gas efficiencies respectively.

In considering the question of efficiency, it is necessary to distinguish clearly between "efficiency" and what may be termed "utility," that is, the suitability to the particular circumstances. For instance, if by-products, such as ammonia, tar, &c., are collected from the gas, the utility of a producer giving large quantities of such products with a poorer gas may be greater than that of a more efficient producer which gives smaller quantities of by-products. If such matters were taken into account, the efficiency of the apparatus would be obtained, considered not as a gas-producer, but as a tar-, ammonia- and gas-producer.

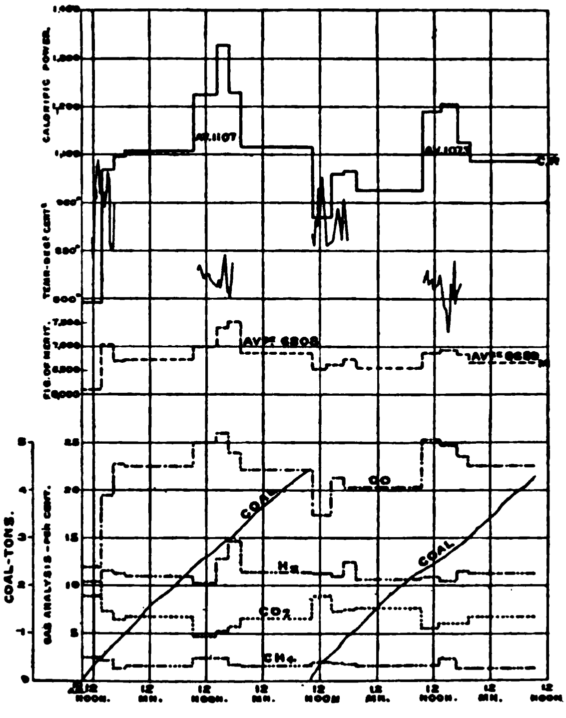
In most cases the efficiency of a furnace burning gas depends, to some extent, on the richness of the gas; this is measured by the "calorific power," the heat of combustion per unit volume, of the gas. This quantity is determined incidentally in the calculation of the efficiency of the gas-producer. In practice the calorific power usually, but not always, varies approximately with the efficiency. A high value for both the calorific power of the gas, and the efficiency of the producer are desirable; and it is well to consider both figures in estimating the merits of a producer. In some cases (p. 11) the calorific power may be of greater importance than the efficiency, for instance, when the gas is used in a gas-engine. In determining the best form of producer for any given circumstances, many other considerations besides its efficiency must be taken into account; but in most instances the Author believes that efficiency should rank as one of the most important, and after any form of producer has been selected it will almost always be desirable to work it as efficiently as possible.

The most direct method of finding the efficiency of a producer would be to measure in a meter the quantity of gas made, and burn samples of the gas and coal in a calorimeter. From these data the efficiency might be calculated at once. The direct measurement, however, of the quantity of gas made is not usually possible, and recourse must be had to chemical analysis to find the relation

between the quantity of gas made, and the quantity of coal burnt. For determining the efficiency of a producer, the average analysis of the gas, the amount of carbon in the coal, the amount of carbon lost with the ashes, the heat of combustion of the coal, and the average temperature of the gas, must be determined by experiment.

The average analysis is calculated from the analyses of a number of consecutive samples of the gas, each collected continuously during

Fig. 1.



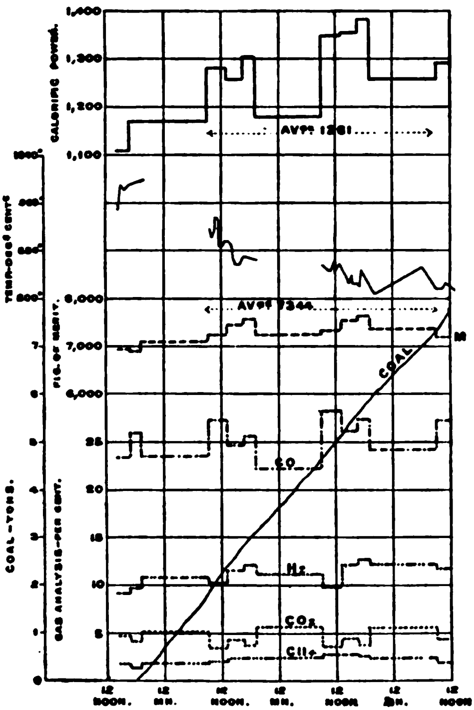
$K = 0{\cdot}75$, $G = 0{\cdot}97$, $H = 8{,}000$, cold-gas efficiency = (1) 0·619 and (2) 0·606.

Results of Tests.

several hours. The total time covered by the series of samples should not be less than twenty-four hours. In calculating the average, account must be taken of the length of time during which each sample was collected. The curves, *Figs. 1* and *2*, represent graphically the results of a few of the tests made by the Author. In each case the conditions of working were maintained as constant as circumstances permitted. The lower lines represent the analyses of the gas, and show by their irregularity

how necessary it is to take an average over a considerable period. The samples must be analysed separately for carbonic acid CO_2, olefiant gas C_2H_4, oxygen O_2, carbonic oxide CO, hydrogen H_2, and marsh gas CH_4. Unless each constituent is obtained separately, the proportion of carbon in the gas and the heat of combustion of the gas cannot be calculated. It is of considerable

Fig. 2.



K = 0·75, G = 0·981, H = 8,000, M = 7,344, cold-gas efficiency = 0·6758.

RESULTS OF TESTS.

importance that in collecting the samples the apparatus should not alter the constitution of the gas by absorption or chemical action. For this reason the water in the aspirator must be previously saturated with the gas which is about to be collected. Herr von Jüptner recommends a layer of oil on the surface of the water. Indiarubber readily absorbs gas, and must only be used in the smallest possible quantities.[1] Composition pipe may be

[1] "Gasanalytische Methoden," by Dr. W. Hempel, p. 2.

used for connecting the aspirator to the sampling pipe in the producer, and as its contents introduce an error, it should be kept as small as possible. It is important that the rate at which the gas is collected should be uniform. The apparatus for gas analysis used by the Author is described in Appendix I, and will give results correct to $\frac{1}{5}$ per cent. of the total gas.

The amount of carbon in the coal is found by elementary organic analysis. Numerous samples should be broken up together and well mixed, a small sample being taken from the mixture as an average. For ordinary coals this figure is about 75 per cent.; but anthracite may have as much as 95 per cent. of carbon.

The amount of carbon lost with the ashes must be obtained by weighing the ashes and the coal during a trial of not less than four days. If the ashes are wetted, the weight of the moisture must be deducted. Samples of the ash must be analysed for carbon, most easily by burning a weighed sample and observing the loss of weight, which must all be due to the combustion of carbon, as the other volatile constituents of the coal have been driven off in the producer. It is essential that the producer should have been working with the same quality of coal for a considerable period before the commencement of the test, so that all the ashes from other qualities of coal previously used may have been eliminated. By dividing the total weight of carbon in the ashes by the total weight of that in the coal burnt during the same period, the proportion of the carbon lost in the ashes is obtained. The magnitude of this figure varies largely in practice. In some producers quantities of coke are wasted every time the producer is cleaned. With open-grate Siemens producers, sometimes as much as 30 per cent. of the carbon is wasted. With good coal and a well-designed producer, this loss may be almost nil. In *Fig. 3* the weight of wet ashes is plotted under the coal line. In this test 1·9 per cent. of the carbon was lost. The proportion of carbon made into gas is the difference between unity and the proportion of carbon lost with ashes.

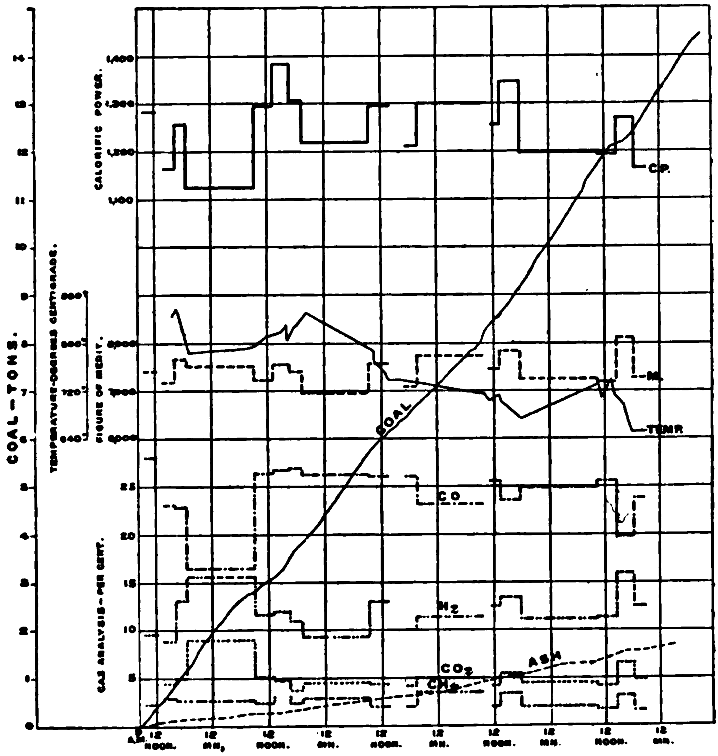
The heat of combustion of the coal may be estimated from its analysis by one of the many formulas which have been proposed. None, however, yield results which agree well with the calorimetric determinations. Some of the formulas are given in Appendix II, but the Author strongly advises the use of the calorimeter. The great value of this instrument has already been pointed out by Messrs. Donkin and Holliday.[1] The Author used for his ex-

[1] Minutes of Proceedings Inst. C.E., vol. cii. p. 292.

periments an instrument supplied by Mr. L. A. Legros. The calorimetric value of the heat of combustion must be corrected for the latent heat of the steam formed, which is included in the result given by the calorimeter, but is neglected in the calculation of the heat of combustion of the gas. This correction usually amounts to about 3·0 per cent.

The temperature of the gas and the extent of its variations

Fig. 3.



are useful guides as to how the producer is working. For this reason a direct-reading instrument for measuring the temperature is preferable to an indirect one, such as a Siemens calorimeter. The Author has used a Le Chatelier pyrometer[1] with great success. With this instrument the variation in temperature from minute to

[1] This instrument is described in a Paper "On the Measurement of High Temperatures," by Professor W. C. Roberts-Austen. Minutes of Proceedings Inst. C.E., vol. cx. p. 156.

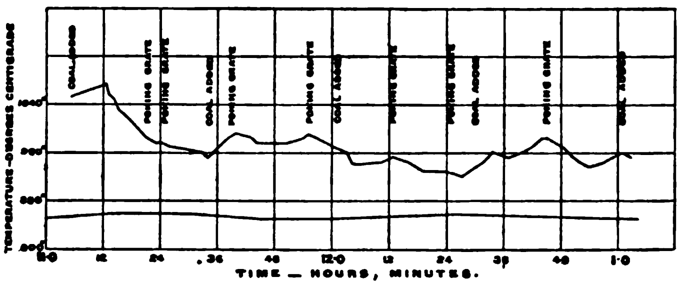
minute may be watched and continuously recorded. The curves, *Figs. 1, 2,* and *3*, show the temperature variations during the trials already referred to. The upper and lower curves in *Fig. 4* show the temperature variations during two hours of a producer working badly and well respectively.

From these data the efficiency may be calculated by the formula :—

$$\text{Cold-gas efficiency} = \frac{M \times K \times G}{H},$$

where M is the heat of combustion of the gas per kilogram of carbon contained in it, K the proportion of carbon in the coal, G the proportion of carbon which is made into gas, H the heat of combustion of 1 kilogram of coal. Of these factors the three latter have already been considered. The first, denoted by M, is of

Fig. 4.



TEMPERATURE VARIATIONS IN PRODUCERS WORKING BADLY (UPPER CURVE) AND WELL (LOWER CURVE).

such importance that the Author has found it convenient to call it the "Figure of Merit" of the gas. It is obtained directly from the volumetric analysis of the gas by the aid of Tables I and II.

TABLE I.—CALORIFIC POWERS OF DIFFERENT GASES.

	Calories per Cubic Metre.
Carbonic oxide (CO)	3,066
Hydrogen (H_2)	2,581
Marsh gas (CH_4)	8,607
Olefiant gas (C_2H_4)	14,045
Carbonic acid and nitrogen	0

The values[1] in Table I are calculated from those given by

[1] The kilogram, cubic metre, and degree Centigrade are used in this Paper as units of weight, volume and temperature. The calorie = 1 Kg C°. The same number measures the "calories per cubic metre of gas," and the "gram C° per litre of gas."

Winkler and Lunge.[1] The water vapour formed in the combustion of the three latter gases is not condensed.

TABLE II.—AMOUNT OF CARBON CONTAINED IN 1 CUBIC METRE OF DIFFERENT GASES.

	Kilogram.
Carbonic acid (CO_2)	0·5376
Carbonic oxide (CO)	0·5376
Marsh gas (CH_4)	0·5376
Olefiant gas (C_2H_4)	1·0752
Hydrogen and nitrogen	0·0000

The calorific power of a gas, divided by the weight of carbon in a cubic metre, gives the heat of combustion of the gas per kilogram of carbon, that is, the figure of merit of the gas. The object of analysing the gas for oxygen is to ascertain whether any air has leaked into the aspirator, but it is doubtful whether oxygen is ever present in producer gas. It is usual to correct the analysis for the quantity of air indicated by the percentage of O_2 found in the gas. The volume of the air is five times the volume of the O_2.

EXAMPLE OF CALCULATION OF THE FIGURE OF MERIT.

Volumetric Gas Analysis.[2]	Calorific Power.	Carbon.
0·040 CO_2	..	0·040 × 0·5376
0·254 CO	× 3,066 = 777	0·254 × 0·5376
0·015 CH_4	× 8,607 = 129	0·015 × 0·5376
0·111 H_2	× 2,581 = 287	.. ..
0·580 N_2	..	.. ..
1·000	C.P. = 1,193	C = 0·309 × 0·5376 = 0·166 kilogram per cubic metre.

$$\text{Figure of Merit} = \frac{1,193}{0 \cdot 166} = 7,180 \text{ calories per kilogram of carbon in the gas.}$$

The importance of the calorific power of the gas has been generally recognized. It has, in fact, been frequently used to measure the merits of the producer. Mr. Dowson, in his Paper[3] in 1882, compares the calorific powers of Dowson gas and coal gas. From the analysis of Dowson gas he calculates its calorific power to be 1,432 calories per cubic metre. The figures given in Table I show a calorific power of 1,321. Mr. Dowson lays special stress

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[1] See also Appendix III.

[2] In many of the following examples percentages are given, the decimal point being then two places further to the right.

[3] Minutes of Proceedings Inst. C.E., vol. lxxiii. p. 320.

on the necessity for rich gas for gas engines. Examples are given in Appendix IV of some rich producer gases.

The great advantage of water-gas over ordinary producer-gas is its higher calorific power. This fact, which has always been insisted on by the advocates of water-gas, has been often understood to imply a higher efficiency for water-gas producers than for ordinary producers, and has given rise to false estimates of the value of water-gas. The Author has not sufficient data to calculate the efficiency of any water-gas producers, but it is almost certain that, even when the producer-gas which is simultaneously made is fully used, the efficiency must be lower than that of ordinary producers; and where the producer-gas is not used the efficiency must be very low. Water-gas is therefore only useful when a high calorific power is of more importance than efficiency.

The analyses and calorific powers of two examples of water-gas are given in the following Table:—

—	Loomis.	Witkowitz.[1]	
	Water Gas.	Water Gas.	Producer Gas.
	Per Cent.	Per Cent.	Per Cent.
CO_2	4·5	1·0	1·7
CO	36·6	46·0	31·7
H_2	57·4	48·0	2·8
N	1·5	5·0	64·8
C.P. . . .	2,630	2,650	1,042

Four cubic metres of Witkowitz producer-gas are made for each cubic metre of water-gas. If the gases were mixed in the ratio of 4 to 1 the resulting gas would have a calorific power of 1,362 and a figure of merit of 7,360.

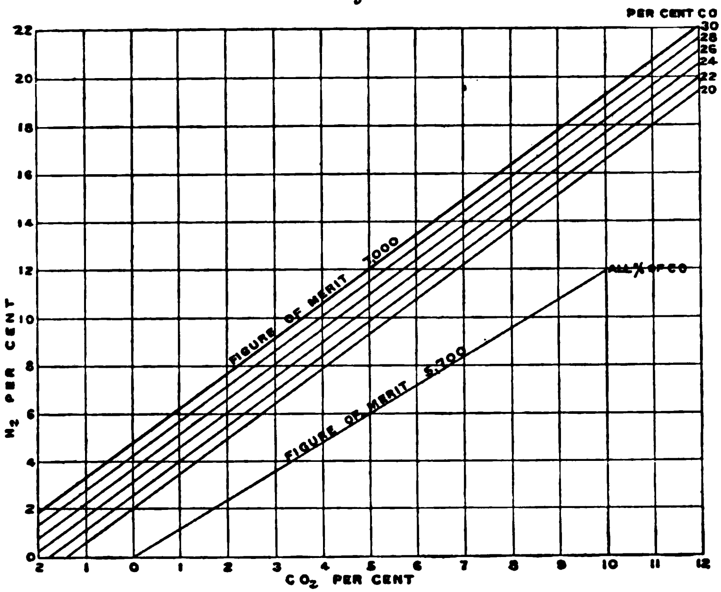
Other figures besides the calorific power have been used to measure the quality of the gases and the efficiency of producers, for instance, the ratio that its combustible elements bear to the whole volume of the gas. This ratio is only approximately proportional to the calorific power and is quite useless for accurate comparisons. The amount of nitrogen in the gas has been supposed to be a good guide as to its quality, but this is misleading. The great importance of the gas-analysis, from which both the calorific power and the figure of merit are calculated, has not hitherto been generally appreciated. The analysis is only of use

[1] Sitzungsberichte des Vereins für Beförderung des Gewerbfleisses, 1888, p. 129.

to enable these two figures to be calculated; yet analyses are usually stated without either of them being given. That it is impossible to compare analyses by themselves is proved by *Fig. 5.* This shows all the different proportions of which a gas may consist to have a given figure of merit. In the gas analyses 0·0 per cent. of CH_4 is assumed.

When experiments are being made on a producer the figure of merit alone is sufficient to measure the quality of the gas, if one kind of coal is used during all the experiments. The second factor of the efficiency, K, depends simply on the coal. The third factor, G, which is of great importance, measures the efficiency of

Fig. 5.



the grate and clinkering arrangements. In experimental work this factor may usually be considered separately from the first. The product of the first and third factors is a complete indication of how economically a producer is working, so long as one quality of coal is used throughout the test. The factors K and H are somewhat more difficult to obtain than M and G; but they need only be determined once for all for any quality of coal which is used in a series of experiments, and, as they depend entirely on the coal, they need not be determined on the spot. A sample of coal may be analysed, and the required figures thus readily deter-

mined. Even when this course has to be adopted the increased value of the experiments will well repay the additional trouble, since by means of these figures the efficiency may be calculated, and the results then compared with others.

The hot-gas efficiency differs from the cold-gas efficiency only because account is taken of the sensible heat of the gas as it leaves the producer, as shown by the formula:—

$$\text{Hot-gas efficiency} = (\text{cold-gas efficiency}) \times \left\{1 + \frac{\text{sensible heat per cubic metre of the gas}}{\text{calorific power of the gas}}\right\}.$$

The sensible heat per cubic metre of the gas = (temperature of the gas − temperature of the atmosphere × the "volumetric specific heat" of the gas). By the term "volumetric specific heat" is meant the heat required to raise the temperature of a quantity of gas 1° C., which, at standard pressure and temperature, would occupy 1 cubic metre. The volumetric specific heat is found from the analysis by means of Table III. The specific heat of all gases except CO_2 is constant. The values of the specific heat of CO_2 for different temperatures are given in Table IV.

TABLE III.—VOLUMETRIC SPECIFIC HEAT OF DIFFERENT GASES.[1]

	Calories per Cubic Metre and C.°
Oxygen (O_2)	0·313
Hydrogen (H_2)	0·305
Nitrogen (N_2)	0·306
Carbonic oxide (CO)	0·306
Marsh gas (CH_4)	0·415
Olefiant gas (C_2H_4)	0·506

TABLE IV.—VOLUMETRIC SPECIFIC HEAT OF CARBONIC ACID.[2]

		Calories per Cubic Metre of Gas at 0° C., and C.°
t = 400	Cp_{o-t} =	0·467
„ 500	„	0·487
„ 600	„	0·507
„ 700	„	0·525
„ 800	„	0·544
„ 900	„	0·562
„ 1,000	„	0·580
„ 1,100	„	0·598
„ 1,200	„	0·615

[1] This Table is calculated from figures given by Prof. Åkerman, *ante*, p. 328 (footnote).

[2] These values are calculated from those given by Prof. Åkerman, p. 86; *ante*, p. 328 (footnote); Cp_{o-t} indicates the mean value of the specific heat at constant pressure between 0° C. and t° C.

The volumetric specific heat of producer gas is usually between 0·31 and 0·33. The temperature lies between 500° and 1,000° C. It is generally sufficiently accurate to neglect the atmospheric temperature. For gas of high calorific power at a temperature of about 700° C., the sensible heat is about 12 per cent. of the heat of combustion of the coal. That is to say, the hot-gas efficiency is about 12 per cent. greater than the cold-gas efficiency. For gas of low calorific power at a temperature of about 900° C., the sensible heat is about 18 per cent. of the heat of combustion of the coal, that is to say, the hot-gas efficiency is about 18 per cent. greater than the cold-gas efficiency. The hot-gas efficiency of a producer is much more nearly constant than the cold-gas efficiency, whether the producer is making good or bad gas. Two examples of the calculation of the hot-gas efficiency are given in Appendix IV.

Most modern producers supply hot gas, but it must not be assumed on this account that the real efficiency of these producers is their hot-gas efficiency. When the gas is used without passing through a regenerator, the sensible heat is all available, and the real efficiency is the hot-gas efficiency; but when the gas is used with a regenerative furnace the case is different, and it seems probable that the sensible heat is almost entirely wasted, the only result being the higher temperature of the chimney gases. If this theory be correct, then for all producers supplying gas to regenerative furnaces the only efficiency which need be considered is the cold-gas efficiency.

The amount of steam in a gas is of considerable importance. Owing to its high specific heat, a relatively small volume of steam carries with it a large quantity of heat. If the gas is used cold, this heat is all lost in the cooling apparatus; if used hot, the steam carries its heat to the furnace where only a portion of it can be used if the furnace rejects the products of combustion above atmospheric temperature as is ordinarily the case. The difference between the efficiencies of the transference of sensible heat by the combustible gases and by the noncombustible dilutants such as steam is remarkable. In the first case all the heat is useful, and the efficiency is 100 per cent., and in the second only a portion is used, depending on the difference between the temperatures in the producer and the chimney. The reason is that the first is concerned with the temperature of a certain quantity of gas which must go to the furnace, and which forms a fixed quantity of products of combustion; whereas the second is concerned with the presence or absence of a dilutant of the gas, which also increases the quantity

of the products of combustion. Steam, besides being an inefficient carrier of heat, produces other ill effects, such as oxidising the iron in a furnace, &c.; the proportion of it in the gas should therefore be kept as small as possible.

Hitherto, no account has been taken of the steam in calculating the efficiency, but the following corrections may be made to include its effects. The weight of steam in the gas per kilogram of coal must be found by experiment. This may be done by drawing a sample of the gas through a drying-tube, care being taken not to allow the steam to condense before reaching it. The heat required to raise the steam from atmospheric temperature to the temperature of the gas must then be calculated by means of Table V, and divided by the heat of combustion of the coal. The result is the required correction to be added to the hot-gas efficiency. A second correction may also be made to allow for the heat supplied to the producer by the steam-jet, if one is used for producing the blast. The amount of steam used in the jet per kilogram of coal must be found by experiment; this may be done by condensing the jet in a tank of water of known weight for a short time. The heat required to raise this quantity of steam from atmospheric temperature to the temperature corresponding to its pressure must then be calculated by Table V, and added to the value of the heat of combustion of the coal (H) used as the denominator of the fraction giving the efficiency. This correction affects both the hot- and cold-gas efficiencies.

TABLE V.[1]—SPECIFIC HEAT OF STEAM (PER KILOGRAM) AT CONSTANT PRESSURE.

$t =$ 0°	$Cp_{o-t} =$ 0·427
„ 100	„ 0·454
„ 200	„ 0·480
„ 300	„ 0·506
„ 400	„ 0·532
„ 500	„ 0·557
„ 600	„ 0·582
„ 700	„ 0·607
„ 800	„ 0·631
„ 900	„ 0·655
„ 1,000	„ 0·679
„ 1,100	„ 0·703
„ 1,200	„ 0·726
„ 1,300	„ 0·750
„ 1,400	„ 0·773
„ 1,500	„ 0·796

These corrections are very troublesome, involving a separate

[1] Given by Prof. Åkerman, *ante*, p. 328 (footnote).

test of the gas and lengthy calculations. The small increase in the efficiency obtained by making them is due to the presence of an obnoxious element which would be much better absent. The gain in efficiency is more apparent than real, since only a much smaller portion of the corresponding part of the heat than of the rest can practically be used. It therefore appears best not to take into account the heat contained by the steam, the error thus introduced being smallest in the best producers. If the gas is used in a regenerative furnace the sensible heat of the steam will be of no value, and it may be actually advantageous to cool the gas sufficiently to condense the steam, so that it will not be present as a dilutant in the furnace where it would lower the maximum temperature, and carry heat up the chimney. This has been pointed out as an advantage possessed by the Siemens cooling tubes.

When a producer has been tested and its efficiency found, it is of importance to know whether the efficiency is as high as it should be, and if not how it may be improved. The first question can only be answered by comparison with the results obtained elsewhere, for which purpose various typical results have been collected in Appendix IV. The following notes and examples will help to show how the particular cause of the inefficiency may be ascertained and remedied.

The "burning of the gas in the producer" caused by the air breaking through the fuel will cause high and rapidly varying gas temperatures. The analysis will show high CO_2, low CO, and low H_2, and low calorific power and figure of merit. Irregularity of temperature is a sure indication of this defect, such as that shown by the upper curve, Fig. 4.

The following analysis represents an extremely bad case:—

	Per Cent.	
CO_2	10·2	
CO	11·8	
CH_4	2·4	
H_2	8·8	
Calorific power		790
Figure of merit		6,020

The defect is probably due either to careless stoking, or to a badly shaped producer which allows the air to find its way up the walls, or to badly placed air-inlets, which should be as nearly central as possible, or to too thin a layer of fuel, which should not be less than 3 feet or 4 feet thick, or to too fast working, though this only produces bad results if carried very far. The speed of

working may be varied within wide limits without bad effects. The ordinary speed is 3 cwt. of coal per hour for a producer of 4 cubic metres total contents. Too much steam produces a somewhat higher value of CO_2 than is necessary, slightly reduces the CO, and largely increases the H_2. The increased value of the H_2 is the most easily distinguished effect. The efficiency and richness may both be high. The following Table shows analyses of gases having a moderate excess of steam, great excess of steam, and as much steam as possible (in order to produce the maximum quantity of ammonia):—

—	Moderate Excess of Steam.	Great Excess of Steam.	Maximum Quantity of Steam.
CO_2 . per cent.	5·30	8·90	15·0
CO . „	23·50	16·40	11·5
CO_4 . „	3·30	2·55	1·9
H_2 . „	13·14	18·60	24·6
Calorific power	1,343	1,202	1,202
Figure of Merit	7,810	8,020	7,840
Temperature .	800° C.	700° C.	500° C.

Excess of steam may not be considered a defect, though the Author believes that the best results will be obtained with the minimum quantity of steam.[1] Too little steam gives rise to a high temperature of the gas. The analysis has the minimum quantity of CO_2, maximum CO, low H_2, high efficiency and richness. The following analysis is taken from Prof. Åkerman's tests. A mechanical blast was used and no steam-jet.

	Per Cent.
CO_2	2·1
CO	27·5
C_2H_4	0·5
CH_4	4·9
H_2	8·3

Calorific power	1,549
Figure of merit	8,110
Temperature	750° C.

The Author has never met with any results with steam-jet producers approaching this in calorific power.

The above are all average samples collected during periods

[1] Prof. Åkerman, p. 157; *ante*, p. 328 (footnote).

of several hours, and similar gases could be produced continuously if the conditions were constant. Occasionally very different gases are produced for short periods, but these could not be produced continuously. The following is an example of a gas from the same producer and coal as examples 1, 2 and 3, (Appendix IV) taken during a few minutes' stoppage, the air inlet being closed.

	Per Cent.
CO_2	9·8
CO	27·9
CH_4	3·6
H_2	31·4

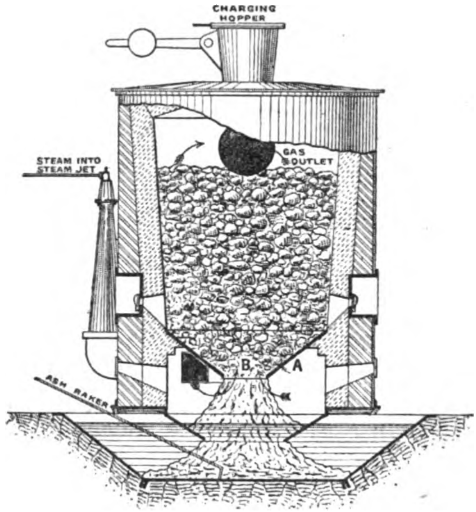
Calorific power	1,975
Figure of merit	8,900

The Author's experiments tend to confirm the statements which have been made by several writers on the subject, that a high temperature of the fuel at the bottom of the producer is advantageous, and leads to the use of a small grate or contracted area where the blast is admitted. This is probably only true when steam blast is used, for Prof. Åkerman's experiments show that very good results can be obtained from large grates where mechanical blowers are used. The Author has found that a blast pressure of about 30 to 40 millimetres of water gave the best results. The best pressure probably depends on the grate-area, depth of fuel-bed, area of producer, &c. The rate of working depends on the pressure employed to a great extent, but for a given rate of working the pressure may be considerably varied by opening up or ramming down the fuel with the poker. With some fuels it is advantageous to ram the fuel down with a flat-ended poker.

A low grate-efficiency is easily recognised, but is not easily corrected. Continuous producers have, as a rule, a higher grate-efficiency than those which require to be stopped periodically for cleaning. Various forms of continuous producers have been designed, some with open water-boshes as at A, *Fig. 6*, such as the Dawson and Shiel producers, and one form of Ingham producer; others with mechanical arrangements for extracting the ashes, as in one form of Wilson producer and the Taylor producer. To obtain a high grate-efficiency the producer must be designed so that all the fuel must pass close to the blast admission, as at B, before it reaches the ash-pit, otherwise some coke will escape unburnt with the ashes. This point has been overlooked in some modern producers.

In conclusion, it is only necessary to call attention to the examples given in the Appendix to show how important a matter the efficiency of producers may be to a manufacturer. Examples are given of efficiencies of 45 per cent. and of 70 per cent. This means that one producer is burning 70 tons of coal while the other only burns 45 tons to give the same amount of heat.

Fig. 6.



CONTINUOUS PRODUCER WITH OPEN WATER-BOSHES.

The second producer would save $\frac{25}{70}$, or more than 35 per cent. of the coal used in the first. The importance of such a saving in the coal bill need hardly be pointed out.

The Paper is accompanied by seven drawings, from which the *Figs.* in the text have been prepared.

[APPENDIXES.

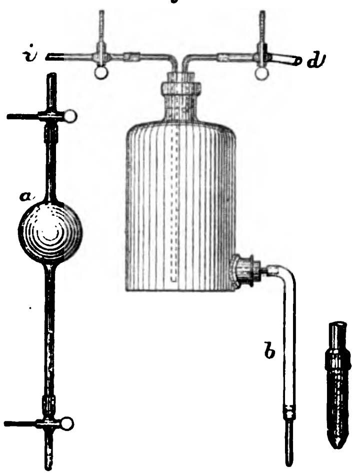
APPENDIXES.

APPENDIX I.

Description of the Gas-Collecting and Analysing Apparatus used by the Author.

To collect the gas samples a glass aspirator bottle was used, fitted with two pipes through a cork at the top and one at the bottom, *Fig. 7*. The inlet pipe *i*, for the gas, passes down to the bottom of the bottle. The pipe for taking the gas out of the bottle is shown at *d*, while *b* is the india-rubber outlet for the water. The flow of water is regulated by a plug in the end of the outlet pipe, made of a short piece of glass tube filled with glass-wool, and nearly closed at the outer end. The glass-wool filters the water and prevents the orifice becoming stopped. This plug acts much better than a screw-clip on the rubber pipe. Several plugs are kept, adjusted for different rates. The aspirator is connected by a small composition pipe to a short piece of porcelain tube which is placed in the gas flue and luted round with fire-clay. The porcelain tube contains a piece of glass-wool to filter the gas. The aspirator is filled in position from a second bottle standing at a higher level through the pipe *b*, the plug being removed, while two samples of the gas are drawn into two bulbs, similar to that shown, through the pipe *d*, for analysis. A dozen or more of these bulbs are kept, each bulb holding about 130 cubic centimetres. The gas samples in the bulbs are carefully cooled to the temperature of the laboratory before the gas is transferred to the burette. The second sample is only used in case of damage to the first in the analysis.

Fig. 7.



The apparatus used for the analysis consists of one double burette, one CO_2 pipette with caustic-potash solution, one C_2H_4 pipette with fuming sulphuric acid, one O_2 pipette with alkaline pyrogallic solution, two CO pipettes with alkaline cuprous chloride solution, one mercury explosion pipette, and one H_2 pipette. This apparatus is fully described and illustrated in Dr. W. Hempel's "Gasanalytische Methoden." It is essential to have two CO pipettes. The Author has found the mercury explosion pipette a more accurate method of determining CH_4 and H_2 than the palladium tube. This apparatus requires a certain amount of skill in its manipulation, and it is necessary to ensure accuracy of the results by analysing known mixtures of gas. The determination of CH_4 is most liable to error, and is of great importance, owing to the high calorific power of this gas. The error, mentioned in the text, which arises from the contents of the pipe connecting the sampling bottle and the producer, may

be avoided, when the gas in the producer is under pressure, by drawing the sample from a branch pipe brought from the producer close past the sampling bottle, and through which a stream of gas is allowed to issue freely. It is not advisable to place the aspirator close to the producer, as changes of temperature affect its regularity of action.

APPENDIX II.

FORMULAS FOR CALCULATING THE HEAT OF COMBUSTION OF COAL FROM ITS ANALYSIS.

I. Used by Mr. von Jüptner in "Oesterreichische Zeitschrift"—

$$\frac{8{,}080\ C + 34{,}462\ H + 2{,}500\ S - 337\ W}{100}.$$

In the first example quoted this gave an error of 7 per cent.

II. A more elaborate formula is given by the same writer in "Die Untersuchung von Feuerungsanlagen," pp. 141, 162.

III. Professor Åkerman used Dulong's formula, but considers it inaccurate—

$$8{,}080\ C + 29{,}000 \left(H - \frac{O}{8}\right).$$

APPENDIX III.

HEAT OF COMBUSTION.

The values for the heat of combustion of gases given by different authorities differ considerably. Much ambiguity also arises from the omission of data as to the temperature and final condition in which the products of combustion are supposed to be left. In the first column of the following Table, the values calculated from those obtained by Favre and Silbermann, as quoted and used by Jüptner, are given. The temperature of the "steam" is not stated. In the second column are given the values calculated from those obtained by Julius Thomsen. These data were kindly furnished to the Author by Prof. J. M. Thomson. The steam is uncondensed, and at the same temperature as the gases before combustion, viz., 18° C. In the third column the figures used in the Paper are repeated for the sake of comparison.

THE CALORIFIC POWER OF GASES.

—	Favre and Silbermann.	Julius Thomsen.	Winkler and Lunge (used in this Paper).
CO	3,014	3,044	3,066
H_2	2,389	2,631	2,581
CH_4	8,498	8,417	8,607
C_2H_4	14,009	13,848	14,045

APPENDIX IV.

Examples of Calculation of Efficiencies.

1. *A Rectangular Brick Producer, Original Form.*

Proportion of carbon in coal	0·750
" " made into gas	0·964
Heat of combustion of coal	8,000 calories per kilogram.
Temperature of the gas	1,000° C.

Analysis of Gas.

	Per Cent.	Calorific Power.	Per Cent.
CO_2	7·5	..	7·5
CO	18·5	566	18·5
CH_4	0·7	60	0·7
H_2	10·4	268	..
N_2	62·9	..	..
	100·0	894	$26 \cdot 7 \times \frac{0 \cdot 5376}{100} = 0 \cdot 144$ Kg.C.

$$\text{Figure of merit} = \frac{894}{0 \cdot 144} = 6{,}210.$$

$$\text{Cold-gas efficiency} = \frac{6{,}210 \times 0 \cdot 75 \times 0 \cdot 964}{8{,}000} = 0 \cdot 562.$$

Hot-gas efficiency is found as follows:—

CO_2	$7 \cdot 5 \times 0 \cdot 580 =$	4·35
CO	$18 \cdot 5 \times 0 \cdot 306 =$	5·66
CH_4	$0 \cdot 7 \times 0 \cdot 425 =$	0·30
H_2	$10 \cdot 4 \times 0 \cdot 305 =$	3·16
N_2	$62 \cdot 9 \times 0 \cdot 306 =$	19·25

Mean specific heat = 0·3272.

Sensible heat per cubic metre of gas = 1,000° × 0·3272 = 327·2.

$$\text{Hot-gas efficiency} = 0 \cdot 562 \left(1 + \frac{327}{894}\right) = 0 \cdot 756.$$

2. *The Same Producer after Modification by the Author.*

Proportion of carbon in the coal	0·75
" " made into gas . . .	0·98
Heat of combustion of coal	8,000 calories per kilogram.
Temperature of the gas	700° C.

Analysis of the Gas.

	Per Cent.	Calorific Power.	Per Cent.
CO_2	5·60	..	5·60
CO	24·60	753	24·60
CH_4	1·25	107	1·25
H_2	17·50	451	..
N_2	51·05	..	..
	100·00	1,311	$31 \cdot 45 \times \frac{0 \cdot 5376}{100} = 0 \cdot 169$ Kg. of C.

$$\text{Figure of merit} = \frac{1{,}311}{0 \cdot 169} = 7{,}750.$$

$$\text{Cold-gas efficiency} = \frac{7{,}750 \times 0 \cdot 75 \times 0 \cdot 98}{8{,}000} = 0 \cdot 712.$$

CO_2	$5 \cdot 60 \times 0 \cdot 525 =$	$2 \cdot 94$
CO	$24 \cdot 60 \times 0 \cdot 306 =$	$7 \cdot 52$
CH_4	$1 \cdot 25 \times 0 \cdot 425 =$	$0 \cdot 53$
H_2	$17 \cdot 50 \times 0 \cdot 305 =$	$5 \cdot 34$
N_2	$51 \cdot 05 \times 0 \cdot 306 =$	$15 \cdot 60$

Mean specific heat = 0·3193.

Sensible heat per cubic metre of gas = 700° × 0·3193 = 223·5.

$$\text{Hot-gas efficiency} = 0 \cdot 712 \left(1 + \frac{223 \cdot 5}{1,311}\right) = 0 \cdot 833.$$

In the first example the hot-gas efficiency is 19·4 per cent. larger than the cold-gas efficiency. In the second example the difference is only 12·1 per cent. This is partly due to the higher temperature in Example 1; but it is also largely due to the much greater volume of the gas in Example 1:—

$$\text{Cubic metres of gas per kilogram of coal, Example } 1 = \frac{1}{0 \cdot 144} \times 0 \cdot 75 = 5 \cdot 21$$

$$\text{,, ,, ,, ,, ,, } 2 = \frac{1}{0 \cdot 169} \times 0 \cdot 75 = 4 \cdot 43.$$

The specific heat is also slightly higher in Example 1 than in Example 2. In the latter the cold-gas efficiency is 15 per cent. higher than in the first example, but the hot-gas efficiency is only 7·7 per cent. higher.

3. *Odelstjerna Producer at Avesta, 1888, with Mechanical Blast (selected from Prof. Åkerman's Tables).*

The following examples are—

Proportion of carbon in coal	0·7617
Grate efficiency	0·9700
Heat of combustion of coal	7,943
Temperature of gas	750° C.

Analysis of the Gas (by Volume).	Per Cent.	*Analysis of the Coal (by Weight).*	Per Cent.
CO_2	2·1	C	76·2
CO	27·5	H	5·1
C_2H_4	0·5	O	8·8
CH_4	4·9	N	1·2
H_2	8·3	Ash	5·1
N_2	56·7	Water	3·2

	Calculated by the Author.	Given by Prof. Åkerman.
Calorific Power	1,549	1,596
Figure of merit	8,110	
Cold-gas efficiency	0·7540	0·774
Specific heat (volumetric)	0·3173	
Sensible heat per cubic metre	238·0000	
Hot-gas efficiency $= 0 \cdot 774 \left(1 + \frac{238}{1,549}\right) =$	0·8690	0·888

The results given by Prof. Åkerman are more correct, as they include corrections for 4 per cent. of limestone which was mixed with the fuel.

4. *Average Results from works at Falun, Avesta, Söderfors, Degerfors, Kolsva and Ankarsrum, with Different Fuels (given by Prof. Åkerman).*

Fuel	Wood.	Wood and Sawdust.	Peat.	Coal.
Heat of combustion of fuel	3,311	2,885	8,402	7,755
Calorific power of gas . .	1,473	1,462	1,472	1,497
Cold-gas efficiency, per cent.	68·6	69·9	66·7	70·3
Hot-gas efficiency „	71·4	72·4	68·0	80·1
Gas Analysis (Volumetric).				
	Per Cent.	Per Cent.	Per Cent.	Per Cent.
CO_2	7·0	7·8	7·1	3·5
CO	27·1	26·0	25·1	27·3
C_2H_4	0·6	0·5	0·4	0·4
CH_4	4·8	4·4	4·3	3·8
H_2	7·1	8·6	11·0	7·8
N_2	53·4	52·7	52·1	57·2
Fuel Analysis.				
	Per Cent.	Per Cent.	Per Cent.	Per Cent.
C	36·4	31·8	36·1	75·4
H	4·6	4·0	3·7	5·1
O	29·5	26·1	18·1	9·9
N	0·1	..	1·2	1·2
S	..	..	..	0·4
Ash	0·5	0·4	9·6	5·4
Water	28·9	37·7	31·3	3·5

5. *Example selected from those given by Mr. von Jüptner.*

("Oesterreichische Zeitschrift für Berg- und Huttenwesen," vol. xxxvi. p. 291 *et seq.*)

Heat of combustion of coal	6,374
Grate-efficiency	0·787[1]
Proportion of carbon in the coal	0·6492
Temperature of the gas	300° C.

Analysis of the Gas (Volumetric).

	Per Cent.
CO_2	4·05
CO	26·00
CH_4	0·35
H_2	12·53
N_2	56·86

Analysis of the Coal.

	Per Cent.
C	64·92
Free H_2 . . .	2·50
N_2	0·50
Chemical H_2O .	14·22
Hygrometric H_2O .	12·42
Ash	5·44

Calorific power	1,154
Figure of merit	7,060
Cold-gas efficiency calculated by the Author	0·5650
„ „ given by Jüptner	0·5405
Specific heat	0·3115
Sensible heat per cubic metre	93·5000
Hot-gas efficiency, calculated by the Author	0·6110
„ „ given by Jüptner	0·5825

[1] This is remarkably low.

OF GAS-PRODU[illegible] Selected

[illegible] coal is

[illegible] showing where [illegible]

[illegible] of gas

[illegible] Also selected [illegible]

[illegible] of combustion [illegible]

[illegible]etric) [illegible]

[illegible]bic met[illegible]

Mr. Dowson gives[1] the following results of a test by Mr. Monaco. The coal contained 92·9 per cent. of carbon, and the heat of combustion (assumed by the Author) was 8,000. The grate-efficiency stated, 90 per cent., is probably low.

Analysis of the Gas.

	Per Cent.
CO_2	8·40
CO	27·50
H_2	16·67

Calorific power 1,272

Figure of merit 6,590

$$\text{Cold-gas efficiency} = \frac{6{,}590 \times 0{\cdot}929 \times 0{\cdot}9}{8{,}000} = 0{\cdot}689.$$

If the grate-efficiency be 95 per cent., the efficiency would be 0·728.

7. *Wilson Producers.*

The following two analyses are given by Mr. Dowson[2] for Wilson gas :—

		Slack Coal.	Durham Coal.
CO_2	per cent.	4·69	4·0
CO	„	23·40	26·8
CH_4	„	2·20	1·4
H_2	„	13·80	11·5
N_2	„	55·80	56·1
Calorific power		1,261	1,238
Figure of merit		7,730	7,140

The following figures refer to producers similar to that shown in *Fig.* 6, The gas sample was collected during a few minutes from the main gas-flue leading from a battery of twenty-four producers, and may therefore be considered as a fair average sample.

Heat of combustion of the coal	7,550
Grate-efficiency assumed to be	0·98
Proportion of carbon in the coal	0·729
Temperature of the gas about	700° C.

Analysis of the Gas.

	Per Cent.
CO_2	8·0
CO	22·0
CH_4	3·0
H_2	14·8
N_2	52·2

Calorific power	1,314
Figure of merit	7,420
Cold-gas efficiency	70·2 per cent.
Hot- „ „	82·1 „

8. *Siemens Producers.*

The following analysis[3] probably represents fairly the gas made in the old open-grate Siemens producers.

	Per Cent.
CO_2	5·9
CO	17·7
CH_4	2·4
H_2	9·8
N_2	64·2

Calorific power 1,003

Figure of merit 6,230

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[1] Minutes of Proceedings Inst. C.E., vol. cxii. pp. 34, 35.

[2] *Ibid*, vol. lxxiii. p. 321.

[3] Taken from Mr. Rowan's Paper, Minutes of Proceedings Inst. C.E., vol. lxxxiv. p. 50, No. 17.

(*Paper No. 2791.*)

"Notes on Weigh-Bridges."

By OSCAR JOHN KIRBY, Assoc. M. Inst. C.E.

A WEIGH-BRIDGE may be so designed as to have either a vibrating or an accelerating motion. A vibrating, or stable, machine is in balance when the pointer or tongue of the steel-yard oscillates slowly between the stops of the carrier until it comes to rest midway between them. An accelerating, or unstable, machine is in balance when the pointer or tongue of the steel-yard rises with an accelerating motion from the lower stop until it is checked by the upper stop.[1] Although greater accuracy can be obtained with a vibrating motion than with an accelerating motion, it is seldom that machines of the former type are erected, as they are much slower in action, and, in the event of the knife-edges becoming blunt, they will lose their sensitiveness to a much greater extent than machines of the latter type.

It is often found in the use of a weigh-bridge, that the oil applied to clean the bright parts of the machine produces adhesion between the steel-yard and the lower stop, varying in amount with the quantity and viscosity of the oil and with the force of impact with which the steel-yard has been suffered to strike the stop. This difficulty is overcome by keeping the under edge of the steel-yard free from grease; and as a precaution a little chalk should be frequently rubbed on the face of the lower stop. Equilibrium must always be indicated by the steel-yard returning to its proper position of rest when brought to the lower stop and then gently released, and by its not rising when the traversing or sliding poise is placed upon the first graduation above zero. It is more difficult to keep a machine balanced when it is provided with relieving or disengaging gear, in consequence of the knife-edges not taking up the same position on the bearings every time the gear is brought into use. On this account it is not frequently fitted to weigh-bridges. The balancing-screw, or balance-ball, as the

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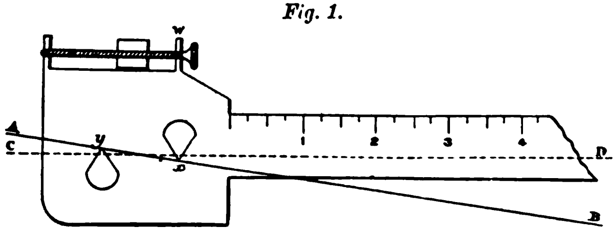
[1] Minutes of Proceedings Inst. C.E., vol. cviii. p. 2.

case may be, is often the cause of much difficulty. The most troublesome form is the rotating balance-ball, especially if it turns freely on the spindle, as its position is easily altered by the impact of the steel-yard upon the stops, causing the machine after a few weighings to be out of balance to the extent of several pounds. A small set-screw with a crutch head should be fitted to a balance-ball of this type, so that it may be clamped in any desired position. But in some machines there would not be sufficient space in the pillar to admit of the set-screw being manipulated. In such cases a back-nut, preferably of a little greater diameter than that of the ball, can be fitted to the spindle of the balance-ball and screwed up tightly to it by hand. Where the balance-weight slides within the steel-yard, and its position is altered by a movable key working a pinion on a rack, a capstan-headed screw, the head of which could be fitted to the key, should be placed in the side of the steel-yard in such a position as to prevent the movement of the balance-weight. In all new machines the balance-weight should be moved in a forward or backward direction by means of a milled-headed screw passing through a fixed diaphragm as at W, *Fig. 1*, or by some other device which will prevent its moving of itself.

A weigh-bridge should be periodically tested by placing forty standard half hundred-weights first upon the centre of the weigh-table and then upon each corner separately. A machine may indicate true weight when the load is placed upon the centre of the table, but when placed at the corners different results may be obtained. This is accounted for by the knife-edges at the corners being worn unequally, the distances between them and the central connection with the main power bearing-lever being thus made either shorter or longer. This irregularity is produced by the resilient motion given to the weigh-table by the horses drawing loads on to it, and by the backward thrust of the revolving wheels of the vehicles. In addition to these forces there are often either lateral or oblique thrusts caused by the horses not taking a straight course when leaving the weigh-table, and in railway-truck weighing-machines by the backward thrust of the revolving wheels and the lateral thrusts of the wheel-flanges. Relieving or disengaging gear causes the same irregularity of wear in the knife-edges, and should on this account not be fitted to a weigh-bridge. These discrepancies can be corrected by regauging the knife-edges to give each lever its true length. Care must also be taken to keep the knife-edges in the same horizontal plane by levelling with parallel straight-edges

and a spirit-level. If the standard weights produce the same result on the steel-yard, whether they are placed on the centre of the weigh-table or upon either of the four corners, the machine should then be tested to its maximum capacity. For this purpose the weigh-table should be loaded with pig- or old iron, stone or any other material ready to hand, to within 1 ton of its maximum capacity, and then the standard weights of 1 ton added. If the steel-yard correctly weighs the added load, the machine can with safety be used to its maximum capacity. Although this may not often be required, it is most important that the test should be made, as a 20-ton machine, fitted with relieving or disengaging gear, may reach its bearings before it is loaded to 15 tons, or even 10 tons, especially if the knife-edges have been packed level with sheet-iron, which becomes soft by corrosion and shakes out of its position. For this reason also a weigh-bridge should not be fitted with relieving gear. The indicating portion of the machine should always be tested; the

Fig. 1.



sliding or traversing poise or poises should be tested separately, and should indicate the same weight on the steel-yard as the standard weight placed upon the weigh-table. If the error of a weigh-bridge does not exceed one two-thousandth of the load weighed on a machine of 5 tons capacity, or one three-thousandth of the load on a machine of 20 tons capacity, or one five-thousandth of the load on a machine of 40 tons capacity, or one six-thousandth of the load on a machine of 100 tons capacity, the requirements of almost any local authority are complied with, provided the machine has been authorised and stamped.

If the steel-yard of an accelerating machine comes to rest before reaching the upper stop, unless the weigh-table be further weighted, the machine is losing power on account of the knife-edges becoming blunted, the equilibrium of the steel-yard being reduced from unstable to neutral. The line A B, *Fig. 1*, shows the position of the centre knife-edges in the steel-yard when the machine is properly in power. In the event of the knife-edges

being worn off to the position of the dotted line C D, the machine would be in neutral equilibrium and completely out of power. The fulcrum knife-edge is shown at x, and y is the resistance or load knife-edge to which is connected one end of the vertical connecting-rod, the other end of this rod being connected to the power end of the main bearing-lever. In vibrating machines the fulcrum and resistance centre knife-edges of the steel-yard are placed conversely, in order to give the steel-yard stable equilibrium, that is to say, the fulcrum is above the centre of gravity instead of below it, as in accelerating machines. As the knife-edges of a vibrating machine become blunt its sensitiveness is being diminished, and the machine is said to be losing its power.

Under favourable conditions a weigh-bridge should maintain its sensitiveness and good working condition for a much longer period than is generally the case; but the dampness of the pit and the ammonia gas which finds its way into it soon combine to produce corrosion of the bearings and knife-edges, with the consequent blunting of the latter. To keep the pit dry, a 3-inch or 4-inch drain-pipe is frequently carried from the pit to the nearest drain or sewer, in some instances a stench-trap being fixed in the drain-pipe. Even where a trap is used its value is to a considerable extent destroyed by the lengths of the periods which may elapse between the changes of the water in it; in fact, it may often happen that for want of water the trap is not sealed. The weigh-house, or office, then becomes a dangerous place, as sewer gas will readily find its way into it from the pit, along the channel of the main power-lever, the heat of the office aiding its ingress. The ammonia gas which comes from the sewer acts injuriously upon all the metal of the machine, but more particularly upon the bearings and knife-edges. It often occurs that the steel-yard of a weigh-bridge is arranged to work inside an office where several clerks are engaged, and the defective drainage arrangements of weigh-bridge pits may be in a great measure accountable for the high percentage of zymotic diseases amongst clerks and storekeepers. In the borough of Batley, Yorkshire, three clerks in one office were in 1892 stricken down with typhoid fever in five months. The drain from the weigh-bridge pit was suspected by the Author and disconnected from the sewer. No case of zymotic disease has since occurred, and the objectionable condition of the air in the office when first entered in the morning has not since been recognized. These drains do not often come under the notice of sanitary officers, or

the dangers from this cause which obtain generally would not exist. When the drain-pipe from a pit has to be connected with a sewer it should be made to discharge itself into the inlet end of a disconnecting trap through which other drainage passes, to ensure the trap being always sealed.

The table of a weigh-bridge is sometimes placed at a higher level than the surrounding ground, to prevent surface drainage entering the pit, and so obviate the necessity of draining it; but unless the weigh-table is under an arch, or is otherwise protected from the weather, it will collect sufficient water to produce fermentation or putrefaction in the pit, which will in its turn make the weigh-office a dangerous place. In the event of the water at any time being deep enough to reach the levers, the weighings will be affected by between $\frac{1}{4}$ lb. and $\frac{3}{4}$ ton, according to the depth of the water, as the levers, when submerged, are partially supported by it, the resistance of the weigh-table being thus reduced. A pit should be properly drained and the air within it kept dry by lime, which should be changed at intervals. The lime must be so placed as to prevent its contact with water. Or if the channel of the main power-lever be carried forward under the floor of the weigh-office to the fire-place, and a grating about 18 inches by 12 inches let into the floor over the channel, the air will be drawn from the pit by the fire unless the office-door or window is open. As an ordinary fire carries about 150 cubic feet of air per minute up the chimney, it is obvious that it can be used for changing the air in a weigh-bridge pit. Many other methods of keeping the pit dry will suggest themselves, according to the variable circumstances connected with the position of the weigh-bridge. In one instance the Author found it convenient to carry the return pipe of a heating apparatus through the pit.

The Paper is accompanied by a tracing, from which the *Fig.* in the text has been prepared.

(*Paper No. 2839.*)

"The Bold Street Extension Tunnel and Central Low-level Station of the Mersey Railway."

By Charles Arthur Rowlandson, M. Inst. C.E.

By the Mersey Railway Acts of 1882 and 1889, powers were granted to the company to extend its tunnel[1] underneath the whole length of Lord Street and Church Street, Liverpool; and, by agreement with the Cheshire Lines Committee, to construct a low-level station immediately beneath the western portion of the Liverpool Central station.

The tunnel between James Street and the Central low-level station rises on gradients of 1 in 31 and 1 in 34, and is, for the most part, in sandstone rock. The portion beneath Ranelagh Place, and as far as the entrance gates of the Cheshire Lines Committee station-yard, is widened into a bell-mouth, having a maximum span of 72 feet 9 inches. The extrados of the arch at this point is barely 15 feet below the surface of Ranelagh Place and is in soft ground, but owing to the special precautions taken with the timbering and packing no settlement occurred. From the entrance gates, and under the station-yard, in which there is a heavy vehicular traffic, the approach tunnel, of gradually widening span, is formed by brick-in-cement side-walls supporting transverse girders carrying jack-arches on which the roadway rests. The side-walls, varying in thickness between 4 feet 6 inches and 3 feet, were carried up in 10-foot lengths and about 6-foot lifts, the excavation being carried upwards from the level of the tunnel and securely timbered. While this work was in progress, the surface of the roadway was removed for the full width, and 14-inch square pitch-pine balks were laid across in trenches, at distances of 10 feet apart. On these balks were placed 12-inch by 6-inch pitch-pine planks, 10 feet long, their ends meeting in the centre of each balk. All the timber had to be set at night, between midnight and 5 A.M., so as not to interfere with the

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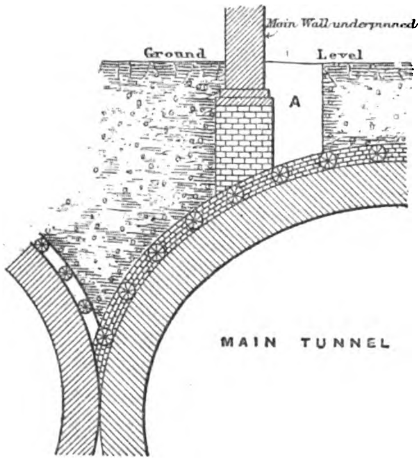
[1] Minutes of Proceedings Inst. C.E., vol. lxxxvi. p. 40.

traffic. The ends of the balks being supported by the finished side-walls, it was then possible to excavate below the planking down to the level of the girder beds, the excavated material being discharged through shoot-holes into the tunnel below. The traffic was thus securely carried on a substantial timber-deck, quite independent of the work beneath. A 10-foot section of the 6-inch planking could readily be taken up during the traffic-less hours of the night, to allow of a girder being lowered, set and replaced ready for traffic by 5 A.M. In this way the girders were set and jack-arches turned and haunched; and when this was completed the decking was removed in convenient sections, and the roadway was reinstated.

The approach to the low-level station, however, passes directly beneath the massive stone building which forms the booking-hall and general offices of the Cheshire Lines Committee railways; and for this portion it separates into three tunnels, slightly divergent, the centre tunnel having double lines and the two outside tunnels each a single line. The western tunnel passes under a heavy cast-iron column, supporting the roof of the central station; and the centre and east tunnels pass directly under the building—the extrados of the centre tunnel arch being only 14 feet below the ground-floor level of the booking-hall. As there are extensive cellars below the building, and the ground was soft and treacherous, extreme care had to be observed in this portion of the work. The centre or main tunnel was driven somewhat in advance of the two outside tunnels. Only 9-foot lengths were taken out at a time, and these were carefully timbered; 15-inch larch bars, and $1\frac{1}{2}$-inch elm poling-boards being used and built in with brick-in-cement packing. In spite of every precaution, some slight cracks appeared in the building, principally around the door- and window-openings. On this account the main walls were underpinned from the arches, as shown in *Fig. 1.*

Fig. 1.

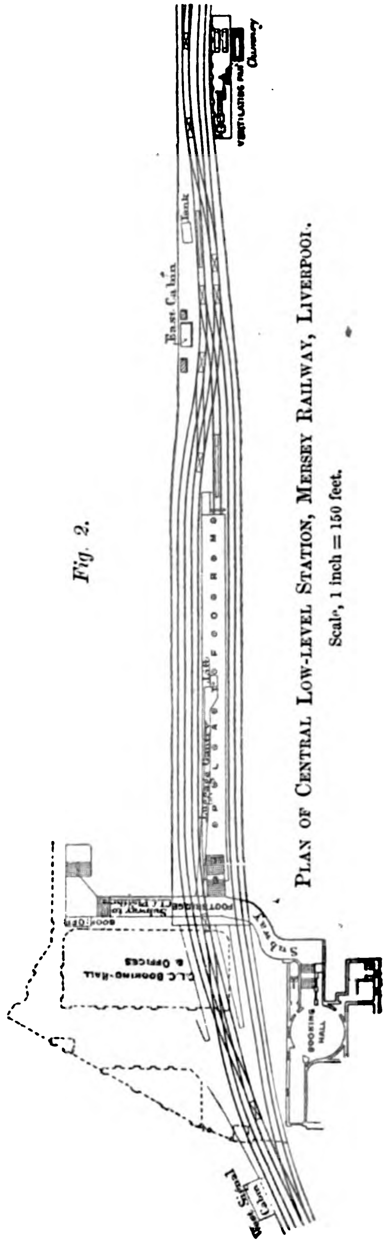


Scale, $\frac{1}{10}$ inch = 1 foot.

Trenches, as at A, 7 feet by 2 feet 6 inches, were sunk alongside the main wall, to be underpinned down to the brick packing on the extrados of the arch. The poling-boards were removed and the underpinning was brought up in 4-foot lifts to the footings of the wall; the last course being always wedged up with thin steel wedges, and cement grout being poured in wherever possible. No further signs of settlement in the building have appeared since. While this work was in hand, three parallel headings were partially driven in the rock beyond, as a commencement of the work of excavation for the station.

A general plan and cross section of the low-level station are shown in *Figs. 2* and *3*. It consists of an island platform with a passenger road on each side, and an engine siding along its whole length on the north side. Beyond the island platform the roads are prolonged into sidings. The station is arranged so that trains can arrive at and depart from each side of the platform; the condensing locomotives on arrival being detached from the train and proceeding to a water-tank to discharge their hot water and refill their condensing tanks. They afterwards pass through the engine-siding to back on to the next train. The station is controlled by

Fig. 2.



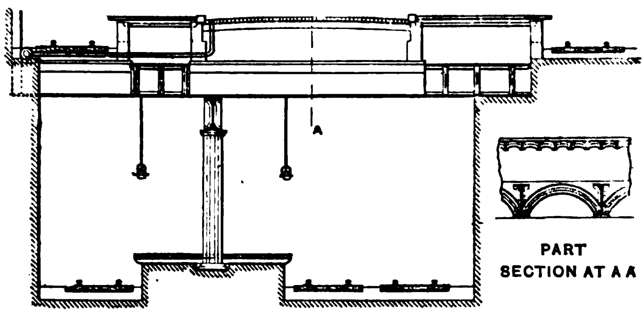
PLAN OF CENTRAL LOW-LEVEL STATION, MERSEY RAILWAY, LIVERPOOL.

Scale, 1 inch = 150 feet.

two signal-cabins, one at each end, and is worked strictly as a block section. Access to the platform is afforded by a staircase leading from a foot-bridge, which terminates in two subways, one leading to Waterloo Place, the other to the platform of the Cheshire Lines Central station. Both subways are provided with booking-offices; and that leading to Waterloo Place contains a large booking-hall and waiting-room, for which the basement of the Lyceum Club was utilized. Hydraulic luggage-lifts are also provided.

The whole of the low-level station is beneath the southern portion of the Cheshire Lines Central station. The portions actually underlaid by the low-level station are the two main arrival platforms, with a cab-rank between them, the No. 1 arrival road next to the south main wall of the station, and some

Figs. 3.



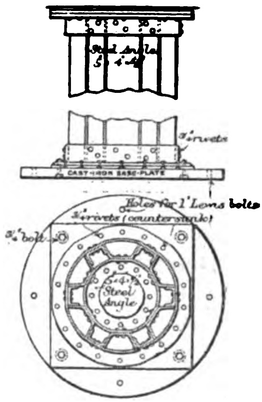
CROSS SECTION OF CENTRAL LOW-LEVEL STATION.

Scale, 1 inch = 24 feet.

of the main roads with points and crossings lying beyond the platforms. These platforms, roads, &c., were built on the sandstone rock which covered the site of the low-level station, and for which a floor of girders and jack-arches had to be substituted. It was of importance to keep the level of the lower station as high as possible, to reduce to a minimum the number of stairs between it and the upper station, at the same time providing adequate height in the low-level station. Consequently, the level of the tops of these girders and jack-arches had to be the same as that of the formation of the Cheshire lines permanent way, *i.e.* only 2 feet below the upper surface of the rails. The clear width of the low-level station at its widest part is 55 feet; but for the length of the platform, 275 feet, this span was divided into two parts by a row

of columns along the centre of the platform, supporting longitudinal girders on which the transverse girders rest. Beyond the platform, columns were inadmissible, and the girders had to span the whole width. These columns are of steel and of the design shown in *Figs. 4*. They are spaced 30 feet apart from centre to centre. The two columns, *Figs. 5*, in the staircase are of oval shape, in order to obtain as much width of gangway as possible.

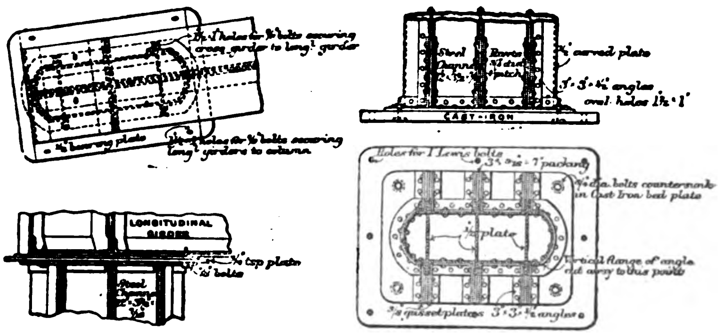
Figs. 4.



Scale, ¼ inch = 1 foot.

The girders throughout the station are made with steel angles and flanges and iron webs, the tensile strength of the steel being not less than 28 tons, nor more than 32 tons, per square inch, with a contraction of the area of fracture of 40 per cent., and an elongation of 20 per cent. in a length of 10 inches. The tensile strength of the iron bars and angles was 22 tons per square inch with a contraction of area of 15 per cent.; and of iron plates, 21 tons per square inch with a contraction of area of 10 per cent. Strips of steel were also tested by heating them to a cherry red, cooling in water at a temperature of 82° F., and bending double to an inside radius of one and a half times the thickness of strip. The whole of the masonry of the

Figs. 5.



Top. Base.

Scale, ¼ inch = 1 foot.

station is laid in cement mortar; the soffit-rings of the jack-arches and tunnels are of brindled bricks.

All the columns, girders, jack-arches, etc., were placed in position and fixed without interfering with the heavy and almost incessant traffic on the platforms, cab-rank and roads of the Cheshire Lines Central station. With the concurrence of the Cheshire Lines Committee the whole of the work was executed from the top, on the same principle as that described for the approach; the conditions imposed being that there must be no interference with the traffic of the Central station, and that all work in any way affecting it must be confined to the hours between midnight and 5 A.M. On these conditions permission was granted to lay timber decking consisting of 6-inch pitch-pine planks, supported by 12-inch square transverse balks, on the surface of the platforms and cab-rank. This work was carried out at night, during non-traffic hours, in 10-foot sections, the transverse balks being spaced 10 feet apart, from centre to centre, the uniform distance decided on for the permanent transverse girders. Trenches were cut in the platforms and roadway, 12 inches deep, for the reception of the balks and the 6-inch planks laid on and secured to them; the difference in level of 6 inches between the permanent roadway and the temporary decking being provided for by temporary timber ramps.

Preparations were simultaneously made from below for the reception and setting of the columns. From a longitudinal heading which had been driven at formation level for the whole length of the lower station, crosscut headings were driven to the positions of the columns, and at these points up-cast shafts were cut vertically to the timber decking. The bed for the foot of the column having been prepared, the section of decking above it was rapidly removed as soon as traffic ceased, the column was lifted off a truck by a portable crane and dropped into its place, and the decking was replaced. The columns having been set and filled with concrete, the longitudinal girders, 30 feet long, were next placed upon them. Trenches or driftways were cut from one column to another to a depth slightly below their tops, and the longitudinal girders were placed in position at night in the same manner as the columns; only that, in the case of the girders, a portion of each transverse balk had to be removed to allow the girder to pass down into its trench. This had been provided for by scarfed joints in the balks, allowing a short length of them to be readily removed and replaced. The cutting of the trenches was of course carried on during both night and day, and quite independently of the traffic, the men working under the timber decking. In the meantime, following up this work, the whole of the rock beneath

the decking was excavated to the depth of the transverse girder beds, the decking being carefully propped, as its supporting rock was removed. In the rear of this excavation the transverse girders were set by the removal of a section of decking at night, in the same way as the columns and longitudinal girders. The jack-arches were then turned and filled in, and the roadway and platforms were reinstated with granite sets and flagging respectively. The excavated rock was shot down into wagons in the headings below, being either wound up a shaft at the eastern end of the works and carted away, or taken through the Mersey Tunnel on to the Wirral Railway and used for bottom ballast.

The operations connected with the setting of the transverse girders were carried on simultaneously at four working-faces, at each of which every stage of progress could be seen in operation from the laying down of the decking to the reinstating of the roadway on the jack-arches. No interference with traffic or accident occurred, and the only sign visible to those using the terminus, of the important works proceeding beneath their feet, was the temporary substitution of a wooden platform or road for the flags and sets. In laying the timber on the flagged platforms, the flags were removed, so that the timber decking was at the same level as the adjoining flags, thus avoiding any necessity for ramps or alteration of levels, which would be objectionable on a platform. As fast as the work was completed and the timber set free, it was carried forward to the other end and used again. It was finally used for sleepers, footbridges, etc., in the low-level station. The turning of the jack-arches was carried on beneath the decking, and without respect to the traffic; but for the filling in of the spandrils and the reinstatement of the permanent roadway and platforms, the decking had to be removed. As soon as the jack-arches were turned, so that the decking could be supported on them, the main body of the rock below was ready for excavation. The latter work was steadily pushed on so as to closely follow the turning of the arches, and the completion of the new floor of the Cheshire Lines station and the roof of the Mersey low-level station.

The use of explosives being inadmissible, the whole of the rock had to be excavated by hand. In character it varied from soft marly sandstone, containing beds of clay and marl between 1 inch and 5 inches or 6 inches thick, to a white rock of great hardness and toughness, which could only be won by the pick, and then slowly and with difficulty. This stratum, however, was of infrequent occurrence and in thin beds, the bulk of the stone being

ordinary sandstone excavated chiefly by the "plug and feather" method. Holes, 2½ inches in diameter, having been bored in the rock by rotary drills driven by compressed air, to an average depth of 3 feet 6 inches, the tapered "feathers" were introduced, and the steel wedge or "plug" was driven home between them by sledge-hammers. The average time occupied in drilling a hole 3 feet 6 inches deep was twenty minutes, and the time occupied in driving home the wedges to the final parting of the rock was about thirty minutes. A set of these tools consisted of two feathers and three wedges of various sizes. With an open face, three sets of these plugs, driven simultaneously, brought down in some cases as much as 8 tons of rock. A careful trial was made of compressed lime cartridges, but with unsatisfactory results, owing to the open nature of the sandstone, and to its being full of thin clay beds, and "backs."

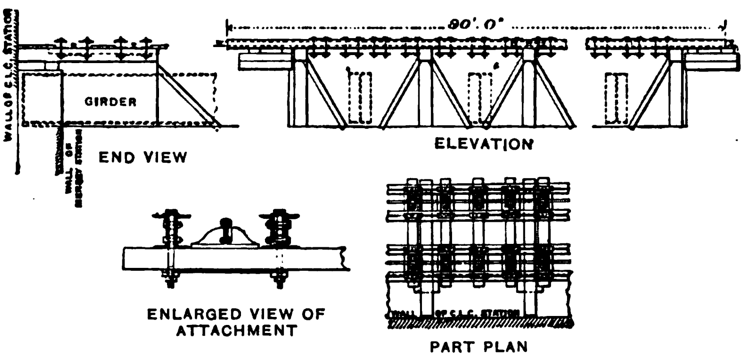
During the progress of the work thin beds of clay and marl were frequently encountered. Where these were wet, and had a slope downwards towards the face of the finished wall of the low-level station, special precautions were necessary to prevent any sliding forward of the rock above the bed; and more particularly along the south wall carrying the main wall of the Cheshire Lines Central station, which forms a retaining-wall for the streets and buildings situated behind it, considerably higher than the station. In these cases the upper block of stone above the bed was securely strutted until the clay bed was cut to a sufficient depth inwards and the cavity filled in with brickwork in cement and carefully grouted. In some places, particularly at the chamber for the ventilating fan, where the rock was somewhat treacherous and supported a heavy load, a facing wall of brick in cement, 3 feet thick, was built up to the full height of the excavation. No slipping or movement of any kind has taken place during the three years since the work was completed. During that period the rock has been exposed to the constant vibration of the trains and engines in both stations, as well as to that of the ventilating machinery in the low-level station.

The cost of excavating the rock varied greatly with the character and position of the work. The most expensive excavation was in the cross-headings driven at the sites of the columns, where the layers of the hard white rock were mostly met with. Here the cost of excavation was in labour alone as high as 15*s.* 10*d.* per cubic yard. The cost of labour in cutting the trenches for the transverse girders was about 11*s.* per cubic yard, but this was reduced to 6*s.* 2*d.* per yard in the trenches for

the longitudinal or D girders between the columns. The cost of excavating the main body of the rock, where it could be reached in open faces and on more than one side, including that of the compressed air for the drills, averaged between 1*s.* 6*d.* and 2*s.* 6*d.* per cubic yard where the sandstone was of ordinary hardness and toughness.

Beneath the No. 1 main passenger arrival line of the Cheshire Lines Central station, on which there is constantly a heavy traffic, a different method of supporting the road had to be resorted to. The formation of the permanent way was solid rock only 1 foot 9 inches below the level of the tops of the rails, and way-beams could not be inserted of sufficient strength to carry the road for the requisite span of 10 feet without cutting trenches

Figs. 6.



TEMPORARY STRUCTURE SUPPORTING NO. 1 MAIN ARRIVAL LINE.

Scale, $\frac{1}{16}$ inch = 1 foot.

for them in the rock under the sleepers, involving taking up the road, and great risk, delay and cost. The way-beams, also, would have seriously interfered with the building and concreting of the jack-arches, the crowns of which are at the formation level, or only about 8 inches below the under side of the sleepers. The difficulty was to carry the road with the heavy engines and trains constantly passing over it, for each clear 10-foot section, by some structure which should not project more than a few inches below the sleepers, nor rise more than a few inches above the rails. It was successfully overcome by an arrangement, *Figs. 6*, designed by the Author from a suggestion of Mr. Francis Fox, M. Inst. C.E. Eight steel channel-bars were bolted together in pairs with distance-pieces between them, and laid on the sleepers, one pair on opposite sides of and as near as possible to each rail. Steel

bolts were passed between the channel-bars on each side of the sleepers to strong plates below them. The channel-bars were in 25-foot lengths, to cover two sections of the work with a sufficient bearing at each end. When the channel-bars were bolted in position, every sleeper for that length was firmly secured to and supported by them; and the transverse trenches in the rock, for the reception of the permanent girders, could then be excavated from below, without interference with the traffic or danger to the miners, although they were in such close proximity to the wheels of the engines and carriages passing immediately above them. While the trenches were being excavated, to a width of about 4 feet or 5 feet, the steel channel-bars were supported by the rock at their ends, and by the rock pillars left between the trenches. When two of the trenches were ready and the girder beds were levelled and dressed, the nearest rail-joints of the permanent way were, during traffic-less hours, disconnected at each end of the channels; and the whole 25-foot length, with rails, chairs and sleepers firmly secured to it, was lifted out by a crane. The timber-decking was then removed, the two girders were lowered into position and set, and the decking-channels and permanent way were replaced. The channel-bars were supported on the top flanges of the girders, and the intermediate rock pillars could then be removed. In the clear spaces thus left, the jack-arches were turned and concreted up to formation level, the road was reballasted, and the steel channel-bars were moved forward to the next two sections of the work. In this way the operations beneath the permanent way were carried on continuously and rapidly, without a single instance of delay or obstruction to traffic or injury to the men.

At the east end of the Cheshire Lines Central station, a portion of the girders and arching lies entirely under the permanent way, which consists of somewhat complicated points and crossings. The use of the steel channel-bars was therefore not practicable, so that in dealing with this portion of the work, it was necessary to insert the girders in two portions, "threading" them in from the side of the roads, which were securely propped from below. The bottom flanges and webs of the girders were riveted together in position, but it was impossible to rivet the top flanges, which were but 4 inches below the timber supporting the permanent way. The top flanges were therefore bolted together with special steel bolts, driven tightly into the holes.

Near the centre of the Cheshire Lines Central station, Newington Street is carried over it by continuous girders, with intermediate supporting columns. One pair of these columns is situ-

ated over the low-level station, near its northern wall, and it was there necessary to underpin them with pillars of solid brick-in-cement, carried up for the full height of the low-level station. These girders, besides carrying the street, support a large portion of the glazed roof of the Central station, upon which any settlement, however slight, might have had injurious effect. The continuous girders were temporarily supported by pitch-pine props of sufficient size and strength, standing on the rock adjacent to the underpinning pillar, and made to take the weight by large folding wedges of steel driven as tightly as possible. These steel wedges were 2 feet 6 inches long by 6 inches wide, and $1\frac{1}{4}$ inch thick on the head, having a taper of 1 in 24. They were driven simultaneously by 14-lb. sledge-hammers; and it was found that, by chalking them, the jumping back was entirely prevented. The rock below the base-plates of the columns was excavated, and the brickwork was inserted in 5-foot lifts to within 12 feet of them. From that point the work was carried on in 3-foot lifts, and only one quarter of the area of the base-plate was excavated and built up at a time, even this portion being carefully and strongly propped. The cement mortar used in this work contained equal quantities of sand and cement, and was always allowed twelve hours to set before any props were removed. For the final grouting under the base-plate, neat cement was used, and when this was thoroughly set, the steel wedges were slackened, and the whole weight was taken by the underpinning pillars. No settlement was observed, nor was there any disturbance of the large area of glazed roof supported by the bridge.

The efficient ventilation of the station was of great importance, as, owing to the large open subways leading into the western end, it was beyond the influence of the existing ventilating fans. Moreover, as it formed the general terminus of the line, it was necessary for every locomotive to discharge the heated water in the condensing tanks, after the completion of each trip, an operation involving the liberation of a considerable quantity of vapour. The object to be attained was to set up a continuous steady current of fresh air, not rapid enough to be recognised as a draught, but of sufficient volume to remove the steam and gases from the locomotives, as fast as they were generated. This current must enter the station at the western end, and travel to the eastern end, where it must escape into the open air. To effect this, a Guibal ventilating fan, 12 feet in diameter, driven by ropes from a compound condensing engine, having cylinders of 14 inches and 22 inches diameter, and 30 inches stroke, was erected in a chamber specially

prepared near the eastern end of the station, steam being supplied from two semi-portable boilers in the chamber. The fan, which is geared to run at twice the speed of the engine, is enclosed in a wrought-iron casing, the air entering freely through the central apertures on both sides, without the intervention of any air-locks or passages. The fan discharges into a rectangular chimney or shaft leading to the Central station-yard, and built as high as the wall surrounding it. The quantity of air moved by the fan is sufficient to maintain a steady gentle current of air from west to east over the whole area of the station, and the ventilation is very satisfactory. To mitigate as much as possible the draughts created in the subways by the entry of the fresh air, inlet apertures are provided in the platform walls of the Cheshire Lines station and at other available points. It was of great importance that a condensing engine should be used for driving the fan, both on the ground of economy and also to avoid the emission of exhaust into the ventilating shaft. The cost of using the town water-supply for this purpose would have been prohibitive, the only practicable source of condensing water being the pumping-station at George's Dock Passage, nearly ¾ mile away. A 4-inch main was accordingly passed down the pumping-shaft at George's Dock, to a depth of 160 feet, and thence through the cross-cut into the tunnel and along the wall through James Street and Low-level Stations to the ventilating-fan chamber, where a well was sunk in the 6-foot way between the sidings. The main is connected directly to the injection-valve of the air-pump. This arrangement works with perfect freedom from any shock or jar, which there was some reason to anticipate from the movement of so large a body of water. The fan, like the others on the Mersey railway,[1] is fitted with a Walker shutter, and works with a total absence of vibration and almost noiselessly.

The extension tunnel and the Central low-level station were opened for traffic on the 11th January, 1892.

The Engineers were the late Sir James Brunlees, Past-President, and Sir Douglas Fox, Vice-President Inst. C.E., and the Author acted as Resident Engineer, assisted by Mr. John Fright.

The Paper is accompanied by four sheets of tracings, from which the *Figs.* in the text have been prepared.

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[1] Minutes of Proceedings Inst. C.E., vol. lxxxvi. pp. 53–57.

(*Paper No. 2845.*)

(*Abstract.*)

"Megass- and Refuse-Furnaces."

By WILLIAM PRICE ABELL, Wh.Sc., Assoc. M. Inst. C.E.

THE use of megass as fuel in the manufacture of cane-sugar has, during the past few years, largely owing to the increased cost of coal, become more general; and on many estates in the West Indies it has entirely replaced coal, of which 25 cwt. were formerly required for the production of one ton of sugar. During the years 1890–94, five hundred and twenty furnaces were rebuilt in British Guiana, at a cost of about £50,000, the annual saving thus effected amounting to more than £100,000.

On the assumption that sugar-cane contains 12·5 per cent. of woody fibre, and the juice 16 per cent. of sugar, Mr. Nevile Lubbock states, as the result of many trials, that double crushing extracts 72 per cent., and single crushing 66 per cent. of the juice, leaving the green megass composed in each case of woody fibre, water and sugar in the following proportions:—

	Double Crushed.	Single Crushed.
	Per cent.	Per cent.
Woody fibre	45	37
Water	46	53
Sugar	9	10
	100	100

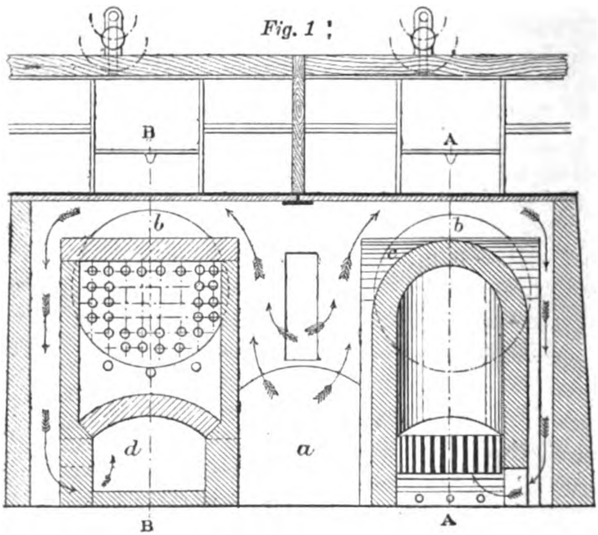
Exhausted diffusion chips reach the furnace composed of 60 per cent. of water, 0·44 per cent. of sugar, and 39·6 per cent. of woody fibre.

In the old furnaces, which do not utilise the oxygen and hydrogen in the water of the megass for combustion, it is calculated that 4·83 lbs. of double-crushed megass and 5·98 lbs. of single-crushed megass are required to give the same amount of available heat as 1 lb. of Scotch coal. Sir Frederick Bramwell and Dr. Letheby, in their report on the sugar manufactories belonging to the Khedive of Egypt, state that 2·09 lbs. of sun-dried megass are equivalent to 1 lb. of Welsh coal.[1] From 100 tons of

[1] See also Minutes of Proceedings Inst. C.E., vol. xlviii. p. 87.

single-crushed cane are obtained 34 tons of megass, sugar and water combined as fuel, $8\frac{1}{2}$ tons of sugar and $57\frac{1}{2}$ tons of water, to be evaporated out of the juice; and from 100 tons of double-crushed cane there result 28 tons of megass, sugar and water as fuel, $9\frac{1}{2}$ tons of sugar and $62\frac{1}{2}$ tons of water to be evaporated out of the juice. With single-crushing, therefore, for every ton of sugar manufactured, 4 tons of refuse or megass, of which 53 per cent. is water, and with double-crushing 3 tons of megass, of which 46 per cent. is water, are obtained. This fuel, in modern though still imperfect furnaces, will evaporate $57\frac{1}{2}$ tons to $62\frac{1}{2}$ tons of water from the juice of 100 tons of cane, besides generating all the mechanical work

Fig. 1.

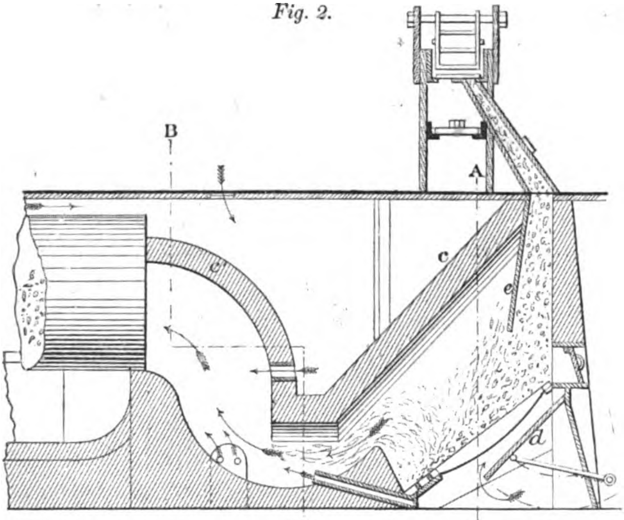


involved in crushing the canes and manufacturing the sugar. A surplus of megass should be stored whilst the mills are working, so that it may be possible to manufacture with it the offal products after the grinding ceases.

The results of long grinding at a factory producing $1\frac{1}{4}$ ton of sugar per hour show that the amount of megass available per hour is 3·75 tons, consisting of 2·03 tons of fibre and 1·72 ton of water. This is burnt in five furnaces, of which two are of the type shown in *Figs. 1* and *2*, and three are of the type shown in *Fig. 3*—4·24 cubic feet of megass being thus available for each furnace per minute; but sufficient steam is maintained with 3 cubic feet per minute. Each furnace has a grate-area of 20 square feet, and each

supplies heat to a multitubular boiler containing 1,300 square feet of heating-surface. The chimney draught, as shown by a Bailey draught-gauge, is 40 feet per second, the flue temperature, as shown by a Bailey pyrometer, is 500° F., and the furnace temperature is such as to melt copper and partially melt cast-iron, about 2,000° F. The steam obtained from these five boilers and furnaces concentrates and purifies 9·45 tons of juice into 1¼ ton of pure yellow sugar per hour. The boiler-pressure is 75 lbs. per square inch, and the cane is crushed at the rate of 13·2 tons hourly, 3·75 tons of megass and 9·45 tons of juice being produced—the latter consisting of 1·25 ton of sugar and 8·20 tons of water and

Fig. 2.



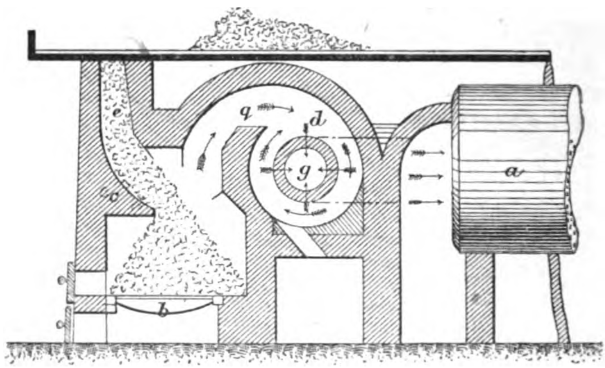
offal. In addition to extracting juice from the canes with a pressure of 250 tons on each of the top rolls, the water is evaporated, the sugar is cured, and the molasses is converted into rum and second sugar. The 3·75 tons of megass develop the requisite power without the assistance of any other fuel, the duty of the engines employed amounting to 463 I.HP. and the heating-surface in the evaporators being 5,650 square feet.

The water in megass was, until a few years ago, evaporated in the furnace and driven out of the chimney as waste steam. In some cases the megass was dried in the sun or in logies, on the theory that to employ water as fuel was to use more energy than could be realised from the combustion of the oxygen and hydrogen

obtained from its decomposition. Ordinary megass, if burnt in the usual way, scarcely produces heat enough to promote its own combustion. The result of the improvements in furnaces has been that condensed steam or white smoke is seldom seen issuing from megass chimneys. Green megass may be taken to consist of 50 per cent. of fibre and 50 per cent. of free water, the small quantity of sugar and ash present being negligible. The chemical analysis of the substance is :—

	Per cent.		Per cent.
Carbon . . .	25·00	Woody fibre . .	50
Hydrogen . . .	2·78		
Oxygen . . .	22·22		
Hydrogen . . .	5·56	Free water . . .	50
Oxygen . . .	44·44		

Fig. 3.

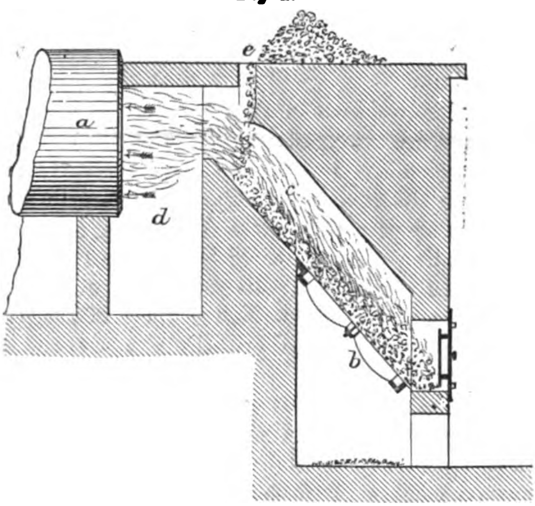


Megass thus contains exactly the amount of oxygen necessary for the complete combustion of its carbon.

When hydrogen and oxygen exist in a compound in the proper ratio to form water, its constituents have no effect on the total heat of combustion. This would be the case with woody fibre in megass, under ordinary circumstances, when burnt in the older type of furnace. On the other hand, in a properly designed green-megass furnace, when thoroughly heated, the water, in the form of aqueous vapour, is decomposed in passing over the highly incandescent and porous carbon of the megass; oxygen is liberated and combines with the carbon to form carbonic acid; and the hydrogen passes off partly uncombined and partly as carburetted hydrogen. The latter, in presence of sufficient oxygen and at the high temperature of the furnace, undergoes further combustion and yields additional heat by its conversion into

carbonic acid and water. This is shown in practice by the fact that little air is required for a furnace after it is properly started. If the pores of megass could be impregnated with air and saturated to the required extent, each atom of carbon being contiguous to the necessary air before the fuel was thrown upon the grate, the result would differ from the ordinary smouldering combustion, and would approach that of gunpowder, which contains the necessary oxygen for perfect combustion without air. The fuel should first be gasified and then burnt, the moisture being split up and the oxygen and hydrogen used for the combustion of the carbon in the megass.

Fig. 4.



In an efficient megass-furnace perfect combustion of the gases and carbon flecks should be produced, the gas being first liberated and then consumed, so that no white vapour escapes from the chimney. It should require no increase of chimney height, and should work without forced draught. The temperature in the combustion-chamber should be sufficiently high for the production of water-gas. Ample provision should be made for expansion through large and varying ranges of temperature. Furnaces have been constructed in which the waste heat from the chimney is utilized for drying the fuel on its way to the fire-box. Experiments made by the Author in 1887, however, showed that with a flue-temperature of 550° F., a layer of megass 3 inches thick took forty-five minutes to dry.

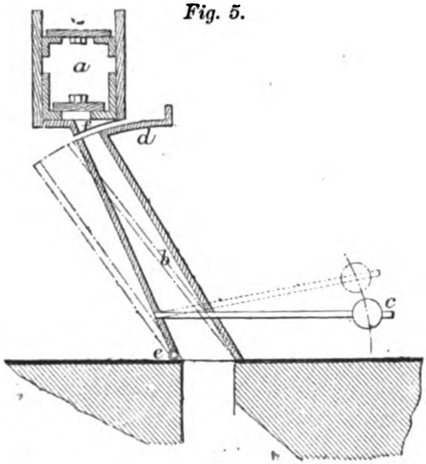
In *Fig. 4* is shown the Alfred Fryer furnace introduced in 1883 by Mr. Maurice Costa, and adopted with success in British Guiana. The grate *b* is inclined with the lowest part at the furnace front, and the green megass is introduced between the bars and boilers *a*, so that all the flames pass through and over the green fuel on its way down the drying-plane *c*. The megass is introduced at the opening *e*, and, falling on the inclined drying-plane, it gravitates downwards on the fire-bars, as that below it is burnt. The whole of the products of combustion pass up over this drying-plane and there evaporate, taking up much of the moisture of the megass. The flames then pass the feed-mouth into the combustion-chamber *d*, and finally into the boiler. No air was observed to be drawn in at the feed-mouth, although the stream of flame was always plainly visible passing this opening, which measured 10 inches by 5 feet.

In 1890 the furnaces shown in *Figs. 1* and *2* were introduced by the Author. They are arranged in pairs, the air for combustion being drawn between the two furnaces at *a*, over the boiler-tops *b*, and finally over the reverberatory arches, *c c'*, into the ash-pit. The air is caused to pass the hottest or lower part of the fire-bars by the deflecting-plate *d*, reaching the top end of the fire-bars and entering the megass at a temperature of 300° F. The check-wall or plate, *e*, regulates the thickness of megass on the fire-bars. In order to still further heat the air and facilitate the complete combustion of the gases and carbon flecks, a similar furnace, but possessing an additional combustion-chamber, was introduced and gave highly satisfactory results. In 1892 the automatic feeding arrangements were added, with a view to reduce labour on megass platforms, to dispense with the gearing hitherto used in mechanical firing, and to give a constant and regular feed of fuel. They have proved so sensitive in practice that without any assistance they can supply one furnace with the exact quantity of megass, passing the surplus on to the next. The megass is carried the whole length of the platform by the usual rake-carrier, connected with each furnace by enclosing hoppers. Down these the megass falls direct into the furnaces until the hopper is sufficiently full to cause the megass in the carrier to pass over that in the hopper. The surplus megass can either be discharged at the carrier end or be stored between each furnace by opening intermediate doors under the control of the attendant. No firemen are required, except when the megass-furnaces are not stopped owing to the mills ceasing work, in which case, by simply opening the door at the bottom of each enclosing hopper, the

furnace can be fed by hand in the usual way. The attendant simply regulates the flue dampers to give the necessary steam, and adjusts the intermediate doors, to prevent too much megass from accumulating at one place. He also sees, by means of the peep-holes in the hoppers, that each furnace is taking its proper amount of fuel.

The apparatus shown in *Fig. 5* utilizes the weight of the megass to regulate its own feed. The hopper is hinged at *e*, *f* being an opening in the cross carrier, *d* a damper or sluice, and *c* a balance-weight. The megass passes down the shoot *b* to the furnace until it is full, and then the weight of the megass, accumulating in the hopper, causes it to descend about the hinge into the position shown by the dotted lines, at the same time raising

Fig. 5.



the balance-weight and sliding the sluice or plate over the opening *f*, thus causing the megass to pass on to the next furnace.

In 1893 a furnace was tried by the Author in which the fuel was passed through an open-ended retort projecting vertically into the combustion-chamber. The flames of combustion heated this retort, and thereby heated, dried and distilled the gases out of the megass ready for combustion so soon as they reached the combustion-chamber, the solid partly-dried fuel falling upon a hearth surrounded by four pigeon-holed walls. It was found advantageous to block up the pigeon-holes until only 7 square feet of grate-area were allowed for burning 7 cubic feet of megass. The ratio was thus 1 square foot of grate-area per cubic foot of megass, whereas it had previously been 6 square feet of grate-area per cubic foot

of megass burnt per minute. In other words, with a boiler having 1,300 square feet of heating-surface, the furnace having one-third the usual grate-area, the amount of steam obtained from double the quantity of megass was three times that derived from similar boilers with ordinary furnaces in the same battery, and this with a natural chimney-draught of 40 feet per second. The construction of the furnaces required only about half the number of bricks of ordinary furnaces.

The loss of megass, and particularly of diffusion chips when charred and partly burnt, is considerable, on account of the unburnt carbon flecks. These are similar in appearance to the end of a charred match, and are carried through the furnace and up the chimney unburnt in such quantities that on a still morning the ground round a sugar-factory becomes strewn with them. To prevent the waste from this cause, a centrifugal combustion-chamber, *Fig. 3*, has been successfully adopted for retaining the carbon flecks until they are distilled and completely consumed. The megass enters at *e* and falls to the firebars *b*. The flames are led into the combustion-chamber *d*, tangentially at *q*, so that the heavy unburnt particles are speedily separated from the light gases by centrifugal force—the heavy sparks and carbon flecks flying off to the circumference, and the light flames of combustion finding their way through the centre *g*, up the flue into the boiler *a*. The same arrangement has been applied with success to the retort type of furnace, wherein the combustion- or whirling-chamber is arranged above the hearth, around which are fixed tangential twyers supplied with air, preferably under pressure, from the chamber. The air enters the twyers and produces a whirling motion which causes the heavier unburnt particles of megass or chips to remain in the chamber, which is of large diameter, in virtue of their weight, whilst the lighter products of combustion pass up the deflector.

The economy of megass-furnaces is still susceptible of great improvement, by the adjustment of the proportions in which the several constituents are consumed rather than by the introduction of new principles of construction or operation.

The Paper is accompanied by several tracings, lithographs and photographs, from a selection of which the *Figs.* in the text have been prepared.

(*Paper No. 2874.*)

(*Abridged.*)

"Mount Bischoff Tin Mine, Tasmania."

By HEINRICH WILHELM FERDINAND KAYSER and
RICHARD PROVIS, Assoc. M. Inst. C.E.

THE Mount Bischoff tin mine is situated in the north-west of Tasmania, where the existence of tin ore was discovered by James Smith on the 4th December, 1871. While prospecting for gold and silver he found a heavy, dark, resinous-looking substance in the bed of a creek; and, presuming from its specific gravity that it was a metallic ore, he carried some of it a distance of 54 miles to Table Cape in order to have it tested. There he learned, somewhat to his disappointment, that the heavy mineral did not contain either of the precious metals, and was simply tin ore. Mining was begun in December 1872, and in August 1873 the present Mount Bischoff Tin Mining Company was formed, with a capital of £60,000, in order to prosecute working on a larger scale. The property consisted of 160 acres, covered for the most part with a dense and almost impenetrable labyrinth of horizontal scrub.

As the carriage of supplies over the existing rough bush-track cost from £24 to £30 a ton, a road to the nearest port, Emu Bay, about 50 miles distant, became a matter of necessity. In 1875 the work was completed, and though, owing to the imperfect method of construction and the excessive rainfall, it was impassable for nine months in the year, it nevertheless enabled goods to be carted to the mine, in the dry season, at a cost of from £6 to £8 per ton. A horse tramway, constructed in 1884 by the Van Diemens Land Company of London, reduced the freight to £5 a ton; and finally its conversion into the present railway, of 3-foot 6-inch gauge, brought about a further reduction to £3 per ton, which is the present tariff except for specially-rated articles.

Geology.—Mount Bischoff consists of contorted and greatly metamorphosed slaty rocks, which are traversed by dykes of

topaz-porphyry and quartz-porphyry. The surrounding plains are largely formed by sheets of basalt. Within an area of 1,100 yards by 660 yards around the mountain the dykes are of a stanniferous character; but beyond this distance they become more compact, harder and sterile. One of the remarkable deposits of tin ore which still form the wealth of Mount Bischoff, is evidently due to the disintegration of portions of the above-mentioned stanniferous dykes.

This is known as the White Face, and is a deposit of detrital tin ore extending over a space 300 yards long from east to west, and 132 yards wide from north to south; it ranges from a thin layer on the slope of the mountain to a bed 70 feet in thickness. The average thickness may be about 25 feet. It contains 2 per cent. to 3 per cent. of cassiterite almost evenly distributed through it; in some parts the constituents are sharp and angular, elsewhere they are well rounded and water-worn. The stanniferous deposit rests upon a layer of micaceous clay, varying between 2 inches and several feet in thickness, under which is a stratum of iron pyrites much decomposed near the clay. It is thought that in the northern part of the deposit, this pyrites rests upon the slate; but in the south it rests upon an older alluvium, consisting of water-worn fragments of chalybite, iron pyrites and blende, with argillaceous matter. This older alluvium covers a considerable area on the south side of Mount Bischoff, and a trial shaft, 35 feet deep, failed to pierce it; it is not, however, stanniferous.

Another important source of supply is the Brown Face. This is a mass of stanniferous "gozzan," and is probably the result of the decomposition, in place, of a large ore-body consisting of iron pyrites and cassiterite; possibly it may have originally formed a highly-pyritic part of a stanniferous dyke. On the east and west it is bounded by stanniferous detritus, exactly similar to that found in the White Face. Underneath, prospecting levels have shown that northwards there is slate; but southwards there is black clay, which, in turn, rests on iron pyrites—the pyrites nearest the clay being in a state of partial decomposition. This face is 253 yards long and 165 yards wide. The workings at present are 36 yards deep and the deposit has been proved to a depth of 87 yards, below which it splits into innumerable thin veins consisting mainly of iron pyrites with tin ore. The face is free from pyrites above the present working floor. The average yield is $2\frac{3}{4}$ per cent. of tin ore, but occasionally rich pockets are met with which yield almost pure cassiterite.

From one of these, 150 cubic yards in bulk, £60,000 worth of ore was extracted.

The Slaughter-Yard Face is similar in character to that last described, but it is somewhat more siliceous. So far as proved, it is 132 yards long by 77 yards wide, the depth ranging from 30 yards to 40 yards, and the average yield being 2¼ per cent. of tin ore. A short time ago some native sulphur was discovered in this face, about 24 feet below the surface, lying under a ferruginous crust. It occurred as small crystals in quartz-sinter, which, in one place, was nearly 2 feet thick.

Several veins traverse the district, but the ore from most of them is highly pyritic, and has to be roasted before it can be successfully dressed. For this reason the tin veins up to the present time have received comparatively little attention; but in the future, when more readily available sources fail, they will no doubt become of greater importance.

The North Valley Lode, situated to the north of the Mount, is likewise worked by the Mount Bischoff Company. It varies between 5 feet and 1 foot in width, and near the surface is free from pyrites; but as a greater depth is attained the ore becomes highly pyritic. The ore occurs only in occasional shoots, which carry at times as much as 50 per cent. of cassiterite; though the average yield, as far as development has yet proceeded, is less than 1 per cent. This lode strikes towards the Brown Face, and, as the deeper of the two adits now being driven will intersect the face at a depth of 293 yards below its outcrop, the work is being prosecuted with vigour.

The Queen Lode is worked by the Mount Bischoff and Stanhope Companies. It lies near the north-east boundary of the Brown Face, and runs through slate and porphyry dykes, in both of which it is productive. It is worked for a length of over 700 feet, the yield being between 15 per cent. and 20 per cent. of free milling tin ore.

Method of Mining.—The three faces, together with such portions of the adjacent porphyry dykes as are worth removing, are worked as open quarries. Ninety-one men are employed in getting and moving crush-dirt from the faces, thirty being miners receiving 8*s.* per day, and the rest truckers and labourers at 7*s.* 6*d.* or 7*s.* per day. In the softer ground each miner excavates about 12 tons of "stuff" per day; but in the hard porphyry dyke only 4 tons. The total cost of mining and delivering the tin-stuff to the company's dressing-sheds is 3*s.* 2½*d.* per ton.

Dressing.—The principal dressing-works are at Waratah, a place

conveniently situated as regards water-supply, about 1¼ mile from the mine. At the mine, the coarser stuff is reduced in a jaw-breaker to a diameter of 2½ inches, and is then deposited in hoppers to which the fine stuff is run direct. From these hoppers the ore is taken in bottom-discharging trucks to the dressing-works, on a railway of 3-foot gauge, with steel rails weighing 46½ lbs. to the yard. This has been worked by a locomotive since 1879, when it took the place of a previously existing horse-tramway. The cost of transport by the horse-tramway was 4¼*d.* per ton; now it is but slightly more than 1*d.* per ton.

Figs. 1, Plate 6, illustrate the general arrangement of the works, showing, however, only a small part of the actual plant. The stuff brought by the locomotive is first passed through stamps, and is then sorted by rising-current classifiers into coarse and fine qualities, which are jigged in two-compartment jiggers. The overflow from the classifiers flows to settling-tanks, from which the slime passes to rotating tables. From these the concentrates are delivered into troughs, and are afterwards re-treated on tables and finally keeved, while the tailings are led to a settling tank and thence to the buddles. The heads from the buddles are re-buddled and finally cleaned in a keeve. The concentrate from the first compartment of each jigger is clean ore ready for the smelter; it is caught in troughs underneath the jiggers. In the second compartment a mixed product collects, consisting of cassiterite with adherent waste; this is removed by a hydraulic-jet elevator and is jigged a second time, while the tailings are concentrated by buddles, the concentrate being pulverized in one of two Chilian mills, then cleaned on rotating tables and buddles, and finally prepared for the smelter by keeves. From the second jigging, the concentrate goes to the smelter as second-class ore, while the tailings go to the Chilian mills above mentioned. The whole of the tailings are reworked in separate sheds, where, after classification, the sand goes to buddles and the slime to rotating tables.

The stamps are of the Californian type; there are three batteries of forty, twenty, and fifteen heads respectively. The moving part of each stamp weighs 5 cwt., the lift is 8 inches, and the number of blows seventy-two per minute. There are five heads in each mortar-box, the order of lift being 1, 4, 2, 5, 3. The screens are of woven steel wire, with 196 holes to the square inch (14 mesh), two to each box; they are 19 inches long by 12 inches wide, and require renewal every eighteen days. The

shoes, which weigh when new 128 lbs. each, have an average life of six months; the dies, of 70 lbs. weight each, require renewal once a year. Although the tin-stuff is not so hard as the ordinary Cornish tin-capels, the water acts corrosively on the shoes and screens, and the crushed ore does not discharge freely on account of its adhesive nature. To facilitate its passage through the screens, only sufficient water is used to produce a good splash, more water being added to the pulp before it reaches the classifiers. During the dry season, when water is scarce, shoes of 64 lbs. weight are used. This method of reducing the amount of ore dressed is better adapted to the arrangement of the works than stopping a part of the plant.

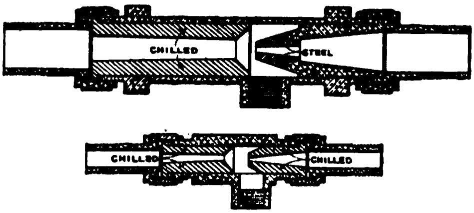
It is necessary to mention that the tin ore is associated with other mineral substances, which have specific gravities only slightly inferior to it, and that therefore careful classification is imperative. Figs. 2 represent the section, end elevation, side elevation, and plan of the double-trough rising-current classifiers. There are in all thirty of these classifiers, and they are used in tandem pairs; the first separating coarse sand, while the overflow carries away the slime. They are constructed of pine boards, 1¼ inch thick, tongued and grooved. Water under pressure is brought into a horizontal pipe under each classifier; the pipe and the classifier being connected by a vertical pipe through which water flows upwards, and the classified product downwards. The latter is delivered into a jigger by a vertical pipe attached to a prolongation of the horizontal pipe. The mouth of the delivery pipe is 12 inches above the bottom of the classifier. In the horizontal pipe, between the classifier and the jigger, is a "gauge piece," which is simply a short iron block, screwed at both ends, and bored with a hole $\frac{3}{16}$ inch in diameter, inserted to prevent an undue quantity of the water going to the jigger. This device is a great improvement on the ordinary cocks formerly used, which suffered so much from the wear of the sharp sand that they became inefficient in a day or two. A wooden plug in the end of the horizontal pipe and the hand-valve A, Figs. 2, are both provided in the event of the appliance becoming choked.

Jigging is performed through the sieve, and there are thirty two-compartment Hartz jiggers. Figs. 3 show plan, longitudinal section through pistons, longitudinal section through sieves and cross section of one of these jiggers. The cases are of 2½-inch pine, tongued and grooved, the whole being held together by ½-inch bolts. The sieves are 2 feet 6 inches long by 1 foot 6 inches broad; woven steel-wire of fine mesh rests for support upon a

stouter sieve, the two being attached to a wooden frame which rests upon an iron grid and is held in position by a bar fixed above it. The pistons are of wood, and are caused to reciprocate by eccentrics, the throw of which can be altered at will. The arrangement is similar to that already described by Mr. E. du Bois Lukis.[1] The connecting-rod is made in two pieces, united by a long coupling-nut, so as to allow easy access to the interior of the jigger without interfering with any other part of the machine.

The hydraulic-jet elevators used to convey the mixed product from the second compartment of the first jiggers to other jiggers (see page 380) are shown in detail, *Figs. 4.* This system of continuously withdrawing the concentrates from the second compartment, and saving the second-class ore separately, enables a better-dressed product to be obtained from the first compartment; it also

Figs. 4.



Scale, 1¼ inch to 1 foot.

acts as an indicator of the regularity of the working of the jigger, any irregularity being shown by a variation in the amount of discharge from the second compartment. The jets work silently and with little attention; the nipple, the only part liable to get out of order, can be replaced in less than five minutes. The jiggers working on coarse sand have sieves with 144 holes to the square inch (12 mesh), and are driven at 160 strokes of $\frac{3}{16}$ inch per minute; while those on fine sand have sieves with 196 holes to the square inch (14 mesh), and make 202 strokes of $\frac{1}{8}$ inch per minute. The sand delivered to the jiggers carries 5 per cent. to 12 per cent. of tin oxide, and the jiggers save between 65 per cent. and 75 per cent. of the output from the Waratah Works; the ratio of second class ore to first class being 1 : 10. It is necessary that both sand and water be fed to the jiggers with the utmost regu-

[1] Minutes of Proceedings Inst. C.E., vol. lxxxv., p. 364.

larity, that the classification be efficient, and that the fragments of ore constituting the bed be of uniform size.

There are thirty-nine convex rotating tables in use for treating slimes. They are similar in principle to the first table described by Mr. R. E. Commans in a Paper[1] recently read before the Institution. Fig. 5 is a sectional elevation of a single table; Fig. 6 represents a double table, partly in elevation, and partly in section; while Figs. 7 show a triple table in both plan and elevation. The surfaces of the tables are now made of cement, which proves preferable to work, on account of the uniform smoothness and evenness obtainable. The frame is of wood, on which is fastened sheet-iron, $\frac{1}{16}$ inch thick, to carry the cement surface, which is 1 inch in thickness; an even edge at the circumference being secured by a tire of $\frac{1}{2}$-inch pine. When the cement is well set, it is strong enough to support its own weight; and, although the sheet-iron foundation corrodes, and in course of time altogether disappears, the efficiency of the table is in no wise impaired. A coating of a mixture, consisting of 25 per cent. turpentine and 75 per cent. coal tar, is laid on with a brush when the cement is sufficiently dry to absorb it. This prevents the corrosion of the surface, which was found to take place before this precaution was adopted. The coating requires renewal only once in two years. The tables vary in diameter between 10 feet and 15 feet, and the slope of the surface is 1 in 12. The slime deposited on the table is swept off at the required points by jets of clean water. The tailings are removed by the first jet, and afterwards the concentrates, two classes being made in the first treatment of the slime, and three in the final treatment of concentrated slime. In order to enable the attendant to vary, as necessary, the point at which each jet of water strikes the table, the nipple is bored at an angle to its axis, so that by slightly turning it round the direction of the jet can be changed. The jets at each table require 23 gallons of clean water per hour. Frequently two or three tables are mounted on one axle; and when space or motive power is limited, this arrangement is to be recommended. In the case of a double table, the concentrate made by the upper table is further cleaned by the lower one, and with favourable ore this may be sufficient treatment before the final tabling; but when the ore is more difficult to treat, a third operation may be necessary, and this is especially the case with very fine slimes.

[1] Minutes of Proceedings Inst. C.E., vol. cxvi., p. 57.

The tables are rotated by worm-gearing once in 2½ minutes; and a single table will treat 7 cwt. of slime per hour, requiring ⅛ HP. to drive it. The double and triple tables consume very little more power than the single tables. The slime as delivered to the rotating tables carries between 0·1 per cent. and 1·0 per cent. of tin ore, and the first concentrates contain between 15 per cent. and 20 per cent. Of the total output from the works, 15 per cent. to 20 per cent. is saved by the tables. The cost of constructing and erecting a single table is £93 at Mount Bischoff, where labour and materials are alike expensive.

There are seventeen Kayser concave buddles. They are similar to those of the Munday type, except that, while in the latter the two scrapers attached to each arm sweep out two separate circles, in the former the two circles overlap, and so enable the process to be more efficiently controlled. Figs. 8 show a sectional elevation and a plan of a buddle. They are between 14 feet and 20 feet in diameter, and, working at a speed of 6½ revolutions per minute, require ¾ HP. each. The slime and sand fed to the buddles contain about 0·25 per cent. of ore. The first buddling raises this to 7 per cent., and by rebuddling the percentage of ore is raised to 60 per cent. The buddles contribute 4·75 per cent. of the ore saved in the works.

Motive Power.—Seven overshot water-wheels, ranging from 18 feet to 40 feet in diameter, supply the necessary power for driving the machinery at the Waratah Works. They are so placed, one under the other, that the water which passes over the top wheel goes to the second, and then to the third, and so on to the last. These wheels furnish 200 horse-power. Overshot wheels are used in preference to turbines or Pelton wheels, because they require less skilled supervision; further, because the water employed comes from other dressing-sheds above and is more or less charged with sand, which would quickly wear out either of the two latter machines.

Lighting.—Electric glow-lamps are employed; a 120-lamp dynamo serving to light the works, workshops and offices. The annual cost, for maintenance only, of seventy-five 16-candle-power lamps is £37 17*s.* 6*d.*; while thirty-seven 12-candle-power kerosene lamps, which were formerly in use for lighting the dressing-sheds, cost, for maintenance, £112 14*s.*

Cost of Dressing.—There are thirty-seven men and boys employed at the Waratah works, thirteen in the day shift, and twelve in

both the afternoon and night shifts. They are employed as shown in the following Table:—

		Per Day. s.	d.
One attending to machinery (including dynamo) and generally supervising the works	at	9	2
Three feeding sixty heads of stamps	„	8	0
One „ fifteen „ „	„	7	6
Three attending to jiggers	„	7	6
Two „ tables	„	5	6
One „ buddles	„	5	6
One „ tables and buddles in slime sheds	„	5	6
One on day shift only, doing general work	„	8	0

The cost of stamping, dressing, bagging, and delivering the ore at the railway is 1*s.* 1⅛*d.* per ton of stuff treated. This includes labour, lighting, oil, grease, repairs and renewals. About 6,000 tons of tin-stuff are treated every month. The dressed ore is sent to the company's smelting works at Launceston, a distance of 160 miles, in two qualities; No. 1 quality assaying, on an average, 70½ per cent. metallic tin, and No. 2·65 per cent. It does not pay to dress cleaner than this, as the cost of extra labour, and the additional losses incurred, would more than counterbalance the increased value of the purer ore.

Tailings and Loss.—After the tailings leave the Waratah works they are again treated, together with those from two neighbouring companies' works, at the "Catch 'em" sheds, ¾ mile further down the stream. Here they are first buddled, then crushed in a Chilian mill, classified and treated on rotary tables. One man and one boy are employed in each shift, and about 50 tons of ore are saved annually. Numerous assays have been made to ascertain the amount of cassiterite in the tailings after they have been finally discharged; but the results differ according to the source of the tin-stuff. The material from the White Face is easily treated, and the loss is extremely small; but it is more difficult to extract all the ore from that yielded by the Brown and Slaughter-Yard Faces—the difficulty apparently bearing some relation to the amount of hæmatite present. Samples taken every day for a fortnight averaged between 0·01 per cent. and 0·2 per cent. of tin, the higher limit being reached only in one sample.

Auxiliary Works.—Until recently a large portion of the alluvium was partially dressed at the mine by sluicing. The concentrate, which contained about 20 per cent. of tin ore, was sent to the Waratah works for final treatment; while the tailings from the

sluices were re-treated at the Ringtail works, situated a little below the mine. Now most of the material adapted for this treatment has been worked out; the sluices are no longer used, and the Ringtail works are only run occasionally to treat the tin-stuff washed down with the drainage from the mine. There is another dressing establishment at North Valley, which, although smaller, only differs in general arrangement from the Waratah works in that it has a calciner for roasting the ore which is pyritic.

Water-Supply.—At the Waratah works, for motive power and dressing purposes, 41,600 gallons of water are used hourly. This is supplied partly by the Waratah rivulet, partly by the Falls Creek, on which extensive reservoirs have been constructed, and the remainder by a small stream called the Fossy.

During the rainy season, these three streams yield a sufficient supply in themselves, but they cannot be depended on after a few weeks of dry weather; and even with such storage as exists, should the dry season be a little longer than usual, there is a deficiency of water. The reservoirs at Falls Creek have been formed by building embankments across the gully. One of these is at present being replaced by a masonry dam, at a cost of about £5,000, which will increase the storage capacity from 165,000,000 gallons to 490,000,000 gallons. The water of the Fossy is conducted to the Falls Creek reservoirs by a race 2 feet 6 inches deep, 3 feet wide, 4½ miles long, with a fall of 2 feet per mile. In all, the Mount Bischoff Company has spent £22,395 in altering the course of streams and in constructing reservoirs.

The inconvenience due to the distance of the mine from all engineering works led to a brass and iron foundry being established at Mount Bischoff in 1887. This has proved extremely useful, both in preventing stoppages and in enabling old material to be worked up again and mixed so as to produce metal best suited to the particular purposes in view. A No. 1 Root blower provides the blast, and also blows four fires in the smithy. A telephone connects the office with North Valley, 3 miles away, the Ringtail dressing-sheds, the mine, and the manager's house.

Mr. Kayser, one of the Authors, undertook the management of the mine in 1875, and the first dividend was paid in February, 1878; but before this desirable stage was reached, £100,000 was expended in developing the mine and equipping it with machinery. Almost the whole of this sum was derived from revenue. Since 1878 the mine has yielded continuous profits, which up to the 30th

June, 1894, had reached a total of £1,500,759. The average price for metallic tin during this period has been £88 15*s.* 3*d.* per ton. The output of dressed tin ore up to the same date amounts to 43,876 tons, extracted from 5,500,000 tons of tin-stuff.

The Paper is accompanied by fourteen tracings, from which Plate 6 and the *Figs.* in the text have been prepared.

APPENDIX.

ROCKS AND MINERALS OCCURRING AT MOUNT BISCHOFF.

White Face.	Brown Face.	Slaughter-Yard Face.	North Valley Lode.	Queen Lode.
Porphyry (eurite).	Hæmatite.	Hæmatite.	Quartz.	Quartz.
Quartz.	Malachite.	Gozzan.	Felspar.	Felspar.
Felspar.	Azurite.	Quartz.	Copper pyrites.[1]	Cassiterite.
Tourmaline.	"Gozzan."	Iron pyrites.	Iron pyrites.[1]	Iron pyrites.[2]
Wolfram.	Quartz.	Fluor.	Mispickel.	
Topaz.	Felspar.	Felspar.	Fluor.	
Cassiterite.	Tourmaline.	Tourmaline.	Talc.	
	Wolfram.	Wolfram.	Cassiterite.	
	Iron pyrites.	Native sulphur.		
	Cassiterite.	Cassiterite.		

[1] Very intimately associated. [2] At deepest points seen.

(*Paper No. 2895.*)

"Plant for the Treatment of Trades Waste."

By William Naylor, Assoc. M. Inst. C.E.

Since the issue in 1874 of the Reports of the Rivers Pollution Commission of 1868, the Treatment of Trades Waste has become of much greater importance on account of the increase of the population and of the growth of industry, together with the more general demand for pure air and water and better hygienic surroundings. The Local Government Act of 1888, by which the County Councils were empowered to administer the Rivers Pollution Prevention Act of 1876, has, in no small measure, contributed to the outcry against the pollution of watercourses by domestic and trades waste; for many of the local authorities empowered with the administration of that Act have been great defaulters, and hence its provisions have not been fully enforced.

Three joint committees of County Councils and County Boroughs, namely, the Mersey and Irwell, the Ribble, and the West Riding, have been formed by a provisional order of the Local Government Board for the purpose of cleansing rivers from objectionable matter. The Manchester Ship Canal is already largely polluted by solid matters brought down the Irwell from the Lancashire manufacturing towns; while the estuary of the Ribble, which is being channelled by means of training-walls, stands in need of protection from pollution by solids from the upper reaches. In an estuary so large as that of the Ribble, liquid pollution will have little effect upon the composition of the water, but the solid pollution not only causes obstruction, but has a stiffening or coagulating action upon the sand of the river. This hardened sand is almost immovable by natural scour.

The following analyses of specimens dried at 100° C. show the difference in composition between the bed of the river in the estuary of the Ribble, the bed upstream, and the mud on the bottom of the Salford dock of the Manchester Ship Canal:—

Sample.	Organic Matter.	Silica and Insoluble Matter.	Carbonate of Lime.	Oxide of Iron.	Alumina and other Metallic Oxides.
	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
Ribble bottom in estuary . .	3·0	96·6	0·4	trace	..
Ribble mud at Walton Bridge	9·8	67·7	12·9	5·1	4·5
Salford-Dock bottom . . .	14·0	77·5	3·6	4·0	0·9

The effect of pollution from manufactories in the second and third of these samples is shown by the high percentage of organic matter and salts of lime and of the metals. The proportion of the silicious matters is higher and of the soluble salts lower in the Ship Canal sludge, owing probably to the acid nature of many of the waste-liquors from factories. In the Ribble, most of the liquors are alkaline, the paper-making industry accounting for the greater part of the pollution.

The forms of pollution may be divided into those from (1) calico and woollen bleaching, dyeing, and printing, (2) paper making, (3) tanning, (4) alkali and soap making, and (5) the galvanizing of iron.

Calico and Woollen Bleaching.—The object of the calico bleacher is to remove as much of the foreign matter as possible from the woven fabric. This consists of the natural resinous, fatty, waxy, colouring and albuminous matter, and that introduced in the sizing of the warps, as well as adventitious dust, dirt and grease. In order to note exactly the extent of pollution caused by each of the several steps in the process of bleaching, samples were obtained and examined by the Author from the Brinscall Bleach and Print Works in January, 1893, with the results shown in the Table, p. 390. It will be seen that the refuse from bleach-works consists principally of liquors holding in solution and suspension starch, lime, greasy and resinous matters, soluble soaps and soluble salts of lime and soda, etc.—mixed with the waste from dyeing and calico-printing operations. The principal polluting agents in calico-printing are: albumen, caseine, china clay, pipe clay, dextrine, glue, gluten, glycerine, gum senegal, gum tragacanth, molasses, lead sulphate, potato starch, salep, shellac in borax, sugar, wheat flour, wheat starch, zinc chloride and zinc nitrate. In addition, therefore, to the contents of the waste liquors from bleachworks generally, are added waste liquors from the dye-becks and the thickenings washed out in calico printing.

Nature of Sample.	Parts per 100,000.							Parts per 100.		Relative Volume.
	Dissolved Solids.		Total Dissolved Solids.	Suspended Solids.		Total Suspended Solids.	Total Solids.	Acidity. Normal NaHO required.	Alkalinity. Normal H_2SO_4 required.	
	Mineral.	Volatile.		Mineral.	Organic.					
										Gallons.
Water-supply from reservoir . .	8·0	10·3	18·3	Nil	Nil	Nil	18·3	Neu	tral.	
First wash (or steep) . .	42·9	145·1	188·0	16·4	55·9	72·3	260·3	0·2	..	20,000
Spent lime from kier .	134·3	613·8	748·1	9·0	105·0	114·0	862·1	..	1·4	1,600
Wash out of lime kier .	22·8	25·7	48·5	6·8	9·7	16·5	65·0	..	0·3	20,000
First (or grey) sour . .	288·9	131·4	420·3	8·8	55·9	64·7	485·0	32·7	..	1,600
Wash out of grey sour .	42·7	22·0	64·7	3·4	10·7	14·1	78·8	1·5	..	10,000
Spent ash liquor (soda) . .	802·1	556·4	1,358·5	(taken with dissolved solids)			1,358·5	..	10·1	1,400
Wash out of ash kier .	30·7	20·2	50·9	3·0	9·0	12·0	62·9	..	2·0	20,000
Spent "chemic".	114·3	29·8	144·1	8·6	11·5	20·1	164·2	..	0·3	1,600
Second (or white) sour	126·5	47·5	174·0	1·2	7·1	8·3	182·3	10·9	..	1,600
Wash out of white sour	8·2	12·1	20·3	2·9	11·2	14·1	34·4	0·3	..	20,000

The objectionable matters in the waste liquors may be either in suspension or in solution. The plant now generally in use for dealing with these liquors may be divided into (1) settling-tanks, to permit of the subsidence of the suspended matter and the precipitation of as much as possible of the dissolved matter, as well as the oxidation by aeration or other means of the remaining portion of the organic matter and the removal of colouring matter; and (2) filters for the further separation of the dissolved and colouring matters by oxidation or other means. The plant shown in Figs. 1 and 2, Plate 7, was designed and set up by the Author at the works of Messrs. John Stanning and Sons, Leyland, Lancashire, and has been in successful operation since January, 1894. The liquors for treatment, dye and bleach mixed, as well as all wash-waters, enter the precipitation-tanks along the carrier from the works after admixture with milk of lime and alumina-ferric precipitants. A dense precipitate of sulphate of lime and aluminium hydrate is immediately formed in the carrier, and the

whole then travels to the first precipitation-tank, which has a division down the centre. About 90 per cent. of the precipitate falls in these two tanks, which are emptied about once per week, the larger tanks being allowed to run for a fortnight or more. The precipitate of aluminium hydrate forms to some extent an insoluble lake with some of the colouring matter, and so clarifies the liquor of dye colorations apart from the deposition of suspended matter. The tops of the divisional walls are formed into carriers with the forward edge about ½ inch lower than the rear, and having a top course of splay bricks for the lips. By this means any forward tank can be missed and the liquors may be passed on by the side carrier running the whole length of the tanks; thus any one, two or three of the four tanks can be used simultaneously, while the fourth is being cleared of sludge. The best results are, however, obtained from continuous flow, due probably to the important fact that the suspended matter in dye and bleach liquor aggregates during slight agitation; although, on standing either in bottles or in tanks for a short time, considerable deposition of the solids and colouring matter takes place apart from any chemical action of the precipitants. The top liquor is conducted away by floating arms, and the sludge is raised by a Shone ejector, into which it gravitates. The tank walls are 3 feet thick, of solid brickwork in cement mortar, and the floors are of 12-inch concrete throughout. The volume of liquor treated per day is 500,000 gallons, that being the capacity of the precipitation-tanks. The precipitated liquors are generally allowed to run into the river direct, but if required for further use they flow into the storage reservoir, Figs. 2. In the latter case they are lifted from the sump A on to the cinder filters, which deliver the water clear and sparkling, and fit for any of the finest bleachwork or delicate shades of dyeing and washing, etc., into the clear-water tanks. Thence pipes convey it from a well to the various departments of the works. The filter-beds are about 5 feet deep, the filtering medium being furnace clinker of various thicknesses. They have not sufficient area to filter 500,000 gallons per day, as it is only necessary to make up a short supply of water from other sources. Should the present water-supply become inadequate, the filter-beds will be enlarged for the treatment of the whole of the flow for use again in the works. The analyses (p. 392) show that the dissolved solids are largely eliminated by the cinder filters.

The quantity of precipitant used is 466 lbs. per day, or 6½ grains per gallon of alumina-ferric. About 10 tons or 12 tons of cloth are bleached per day, the cloth, reckoned dry, showing on the

average 10 per cent. of size to be abstracted in the process. This amounts to 1·1 ton per day of dry, or 22 tons of wet sludge, the whole reckoned as in suspension and the sludge containing 95 per cent. of water. But to this must be added 1¼ ton of starch, lime, soda and other matters passing from the works to the tanks as waste, making a total of 47 tons of wet sludge per day. Of this only 33 tons are retained as sludge, the remainder passing away in solution in the liqours. The wet sludge is lifted by the ejector to a height of 17 feet and is delivered into earth drying-pits, from which it is carted away when dry. The ejector has a capacity of 100 gallons, and the air-compressor has 6-inch steam- and air-cylinders, with a 9-inch stroke. Working at a steam-pressure of about 50 lbs. per square inch, it is capable of dealing with 100 tons of wet sludge in twelve hours. Tested on the 21st November, 1894, the steam-cylinder of the air-compressor indicated 1·75 HP. with a pressure of 10 lbs. per square inch in the air-receiver, and a piston-speed of 150 feet per minute. The diagram taken at the same time from the air-cylinder indicated 1·1 HP., the steam-cylinder running with a light load of 0·76 HP. The compressor was working two hours and twenty-seven minutes, and ejected 11,800 gallons, or 63·2 tons, of sludge.

The following analyses of samples drawn during a period of six hours, at intervals of fifteen minutes, expressed in parts per 100,000, show the effect of the treatment:—

Description of Sample.	Dissolved Solids.			Suspended Solids.			Total Solids.			Remarks.
	Volatile.	Mineral.	Total.	Organic.	Mineral.	Total.	Mineral.	Organic.	Total.	
Raw liquor . . .	44	86	130	24	12[1]	36	98	68	166	Highly discoloured.
Tank „ . . .	40	92	132	..	..	..	92	40	132	Clear.
Filtered „ . . .	10	56	66	..	..	..	56	10	66	Clear and sparkling.

The composition of the mineral dissolved solids in each case was:—

—	Common Salt.	Silica.	Sulphate of Lime.	Chloride of Lime.	Sulphate of Iron.	Sulphate of Alumina.	Alumina.	Oxide of Iron.	Carbonate of Lime.
Raw liquor . . .	36·3	15·8	13·2	8·9	7·3	3·5	3·1	..	..
Tank „ . . .	34·1	..	45·3	6·1	..	..	2·8	3·0	1·6
Filtered „ . . .	14·5	14·4	21·8	..	0·56	..	trace	trace	4·7

omposed of 11 parts of carbonate of lime and 1 part of oxide of iron.

It will be seen that although precipitation removes all the suspended solids, no difference is perceptible in the amount of dissolved solids. The raw liquids entering the tanks are discoloured, muddy, and contain suspended matter. They emerge from them clear, but little different so far as dissolved solids are concerned. The filtered liquor, however, is considerably improved, both as regards suspended and dissolved solids, containing only 46 grains of solids per gallon. The sludge, when dried at a temperature of 212° F., gives the following analysis:—

	Per Cent.
Organic matter	19·6
Silica, &c.	38·5
Calcium carbonate	31·4
Alumina	3·2
Ferric oxide	7·3
	100·0

The calorific power of the organic matter present is so low that the sludge is valueless as a fuel, for when placed in a calorimeter tube with a fusion-mixture it will not ignite but is converted into a liquid mass. When powdered and thrown on a boiler-furnace it gives for a few minutes a lambent blue flame with little heat, and then settles into a solid mass of clinker, which it is almost impossible to fire in ordinary furnaces. As a manure also it is valueless, the percentage of phosphates and ammonia being exceedingly low. If burnt to a clinker, it might be used for mortar-making, but other substances equally good can be obtained at a cheaper rate.

A series of precipitation-tanks, similar to those shown in Fig. 1, have been put into operation at Messrs. Fox's Woollen Mill, Wellington, Somerset. The supernatant liquor is, however, raised by pumps, owing to the levels not being suitable for it to be siphoned off by floats. As much more soap also is used, these liquors are treated separately and the soap is recovered.

Soap is formed by the combination of a fatty acid with a base, which may be an alkali, an alkaline earth, or a mineral. The fatty acids of a soap may again be separated and recovered, either by treating it with an acid which has a greater affinity for the base than for the fatty acids, in which case the latter are liberated; or by treating it with a second base, such as lime, which has a greater affinity for the fatty acids than has the base already combined with them, when a second soap will be formed, which, if insoluble in water, can be collected as a precipitate.

The first of these methods is that in use at the Wellington mill. The soap-liquors are conducted into six acidifying tanks, along with sufficient acid to liberate the fats, which on separation are drained on sawdust filters. Later they are taken off and purified by distillation for use again. The resultant or mother liquor from these acidifying tanks is pumped into the intermediate storage-tanks for further precipitation and settlement, after which it gravitates into the precipitation-tanks proper, and is treated with the general waste liquors by alumina-ferric. The volume of liquor treated per day is about 600,000 gallons, and the tanks have a capacity of 300,000 gallons. Average samples of the liquor were taken during three or four days in January, 1895, and the effect of the treatment, expressed in parts per 100,000, is shown in the following Table:—

Nature of Sample.	Dissolved Solids.			Suspended Solids.			Gross Solids.		
	Mineral.	Volatile.	Total.	Mineral.	Volatile.	Total.	Mineral.	Volatile.	Total.
Raw liquor	46	18	64	18	8	26	64	26	90
Treated liquor	24	12	36	2	..	2	24	14	38

The raw soap-liquors contained, on an average, about 500 parts in 100,000 of solids, of which the fats averaged 150 to 250 parts, but in the treated soap-liquors no fats were present. The sludge, which is lifted from the wells on to cinder filters, amounts to about 10 tons per day, and the precipitant to 392 lbs. per day, or 4½ grains per gallon. The sludge gives the following analysis:—

	Per Cent.
Moisture	76·40
Sand and silicious matter	14·50
Organic matter	3·98
Lime as CaO	1·39
Alumina as Al_2O_3	0·60
Ferric oxide	0·52
Sulphuric anhydride	0·34
Combined chlorine	0·02
Carbonic acid	1·56
Soda, &c.	0·69
	100·00

The Mather-Platt method of treating bleachers' and dyers' waste, adopted at Messrs. Rylands and Sons' Bleach Works, Chorley, is shown in Figs. 3 and 4. There are four tanks, at the bottom of which is laid a series of aeration pipes M, through which air is drawn by means of a steam injector. The precipitant is also blown in by a series of pipes L about 1 foot above the aeration pipes, so that the sludge is not disturbed when it is added. After the addition of the precipitant, the contents of the tanks are blown up for some minutes into a seething mass, and are afterwards allowed to settle. The precipitants are mixed in wooden tanks, shown on the divisional walls, with water drawn through the dilution pipe K and agitated by air also introduced by the injector. The sludge runs from each of the four tanks into a chamber shown in the sections AA and BB, from which it is lifted by a centrifugal pump.

In filters, the decolorization of dyers' wastes is probably not effected so much by oxidation as by the actual abstraction of the colouring matter. The filtering medium abstracts the colouring matter, as evidenced by the satisfactory results obtained so far as this point is concerned with a filter of straw, and also by the complete change of colour of the filtering media in all dyers' filters, as well as by the falling off in decolorizing efficiency after continued use. On the other hand, in precipitation-tanks the decolorization is more probably due to the presence of iron or aluminium hydrates liberated from the precipitant, which form an insoluble lake with the colouring matter. The amount of decolorization which followed the use of precipitants of iron and alumina without aeration for twenty minutes, has been shown by the Author's experiments to be much greater than that which ensued from three or four hours' aeration without precipitants.

The principal advantages of the aeration system are: The exceedingly quick and intimate admixture of the precipitants and liquors under treatment; the accuracy and ease with which precipitants can be used, and the results gauged as regards alkalinity, acidity or neutrality; and the reduction in the amount of precipitant used owing to the admixture of sludge already precipitated. With regard to the latter, it is certain that all finely divided precipitates are caused to aggregate by violent agitation; and, so far as the Author has been able to observe, the rapidity of aggregation is in direct proportion to the amount of precipitate present, the degree of agitation being constant. This is exemplified in the

case of a finely divided precipitate of calcium oxalate or barium sulphate, which aggregates on aeration or boiling; as is also the case with turbid argillaceous water, which will settle quite clear on agitation with a small quantity of sand. The same phenomenon is also noticeable if the liquor containing the precipitate be allowed to pass through a series of U-tubes. Aggregation takes place principally when the liquor is rising, and this in some measure accounts for the efficiency of the Candy tank, Figs. 5.

At various works where this system has been adopted, both alumina-ferric and lime, and copperas and lime, have been tried; but for bleachers' waste only, copperas and lime give better results than alumina-ferric and lime. The results obtained during an extended trial of the system in March, 1895, are given in the following Table, expressed in parts per 100,000:—

Nature of Sample.	Dissolved Solids.			Suspended Solids.			Total Solids.			Remarks.
	Mineral.	Volatile.	Total.	Mineral.	Volatile.	Total.	Mineral.	Volatile.	Total.	
Raw liquor . .	52	20	72	..	10	10	52	30	82	Reddish-black colour.
" " after treatment . .	76	24	100	..	..	..	76	24	100	Colourless.

The amount of precipitant used varies with the liquor to be treated, but in this case 300 lbs. of lime and 300 lbs. of copperas were required per 100,000 gallons.

The two cases mentioned are exemplary of waste arising from the heavy bleaching of goods containing a considerable amount of size, say 20 per cent. or 30 per cent. Cloth, however, woven for purposes of printing (and yarn also) generally contains considerably less than this amount, between 8 per cent. to 12 per cent.; and therefore the refuse liquors are cleaner, on account of a smaller weight per ton of matter to be abstracted and a smaller weight of reagents used in the abstraction. The washing waters are nevertheless as voluminous as in heavy bleaching, owing to the finish required, the resulting waste being comparatively clean —containing about 10 parts to 20 parts per 100,000 of suspended solids and 50 parts to 60 parts of dissolved solids. Experiments made with the filtration of this class of liquor show, that the dissolved solids are only slightly affected when less than 50 parts

or 60 parts per 100,000; and, as these are principally unobjectionable mineral matter, it is doubtful whether further treatment than the interception of the suspended solids is necessary. The filters were 6 feet thick, and were first thoroughly washed. The following Table shows the result of filtration on the dissolved matter in bleach waste, expressed in parts per 100,000 :—

Initially.	After Filtration through Sand.		After Filtration through Cinders.	
	500 Gallons per Square Yard in 24 Hours.	200 Gallons per Square Yard in 24 Hours.	500 Gallons per Square Yard in 24 Hours.	200 Gallons per Square Yard in 24 Hours.
130	Filters choked	Filters choked	85	69
70	59	48	63	57
40	34	30	36	32

The desired elimination of the suspended matter can be effected more cheaply by a subsidence- or a precipitation-tank than by filters, which, with any appreciable amount of suspended matters, require frequent washing. For this purpose the Candy self-cleansing tank has proved itself to be well adapted. It is better fitted for liquors containing small percentages of suspended matter than are the horizontal tanks generally in use. That shown in Figs. 5 is fed at the bottom by down-pipes A, the inlets being laid at semi-tangents B, to avoid the disturbance of sludge already deposited. The liquors ascending to the surface pass away in thin films, void of suspended matter, along the channels C laid about ½ inch below the outlet.

The Ives tank has a cone-shaped bottom, the sludge being pumped from the apex, under the liquor. It is found, however, that only the sludge immediately above the suction-pipe is drawn out, the greater portion of it remaining on the sides. The Candy tank has a flat bottom from which the sludge is siphoned off by a perforated pipe D moved over the surface by a winch. The sludge is delivered about 2 feet below the water-level and is of a satisfactory consistency. The following results have been obtained with this tank. The number of gallons treated was 100,000, the time occupied being twelve hours, and 6 grains per gallon alumina ferric being used as precipitant.

Before Treatment.						After Treatment.					
Dissolved Solids.			Suspended Solids.			Dissolved Solids.			Suspended Solids.		
Mineral.	Volatile.	Total.	Mineral.	Organic.	Total.	Mineral.	Volatile.	Total.	Mineral.	Organic.	Total.
43·1	35·1	78·2	3·6	15·8	19·4	50·6	21·3	71·9			
38·2	21·9	60·1	4·7	20·6	25·3	42·7	18·6	61·3			
44·3	41·7	86·0	3·2	13·9	17·1	47·3	33·3	80·6	..	2·0	2·0

At the works of the Pincroft Dyeing and Printing Company, at Adlington, Lancashire, the waste liquors containing indigo in solution are passed into precipitation-tanks, receiving on the way between 10 grains and 15 grains per gallon of calcium hydrate. The precipitated indigo is raised by an injector from the bottom of the tanks, and is blown into the dye-vats for use again. All other liquors, such as aniline-dye liquors, etc., used in the works, are treated in separate tanks, the precipitants being lime and ferric chloride. The raw indigo liquors before treatment contained, on the 17th January, 1895, 0·4 part of indigotine per 100,000, or 4 lbs. per 100,000 gallons. They are then of a deep blue colour, but after precipitation are clear and transparent, containing a reasonable amount of dissolved solids with little objectionable organic matter. Results of examination of liquors from the Pincroft Dye Works, expressed in parts per 100,000, are given in the following Table:—

Nature of Sample.	Dissolved Solids.			Suspended Solids.			Gross Solids.			Indigotine.
	Mineral.	Volatile.	Total.	Mineral.	Organic.	Total.	Mineral.	Volatile.	Total.	
Indigo waste before treatment	90	34	124	16	30	46	106	64	170	0·4
Indigo waste after treatment	84	30	114	3	1	4	87	31	118	..

Wool-washing Waste.—In the washing and scouring of wool a quantity of fat or grease is liberated, for which a special process

of treatment is required. The object of scouring wool is to remove natural impurities, such as yolk, dirt, oil, etc., and to leave a clean surface for dyeing. The wool contains large quantities of potash salts which may be recovered before scouring.

At the works of Messrs. Thomas Biggart, of Dalry, Ayrshire, the recovery of the grease and potash from the liquor produced in scouring the wool is effected in the following manner. The suds from the first bowl, containing about nine-tenths of the grease and potash, after standing about twelve hours to ensure deposition of the sand, are evaporated in a pan until the liquid assumes the consistency of syrup. It is then cooled on shallow iron trays, the grease, which collects at the top, being removed at intervals. The semi-liquid residue, containing the potash and organic matter, is calcined in a brick oven, the heat being used to assist in the evaporation. A crude carbonate of potash is produced in the calcining chamber, which, after being completely carbonated, is boiled to dissolve out the potash salt. It is then concentrated to 100° Twaddell at 212° F., and the sulphate and chloride of potassium crystallizes out on cooling. The quantity of fuel required is about 1 ton for 1,500 gallons; the carbonate of potash can be sold at £16 10*s*. per ton, and the grease, when boiled with vitriol, at £60 per ton.

In France and Belgium, and occasionally in England, the potash is extracted from the wool before it is scoured with soap. In a recent type of machine, that of Mr. Emile Richard-Lagerie, of Roubaix, the wool, in a series of tanks with distributors and automatic valves, is subjected successively to the action of liquors of diminishing strengths, the last being clear water for rinsing. The liquor, after having passed through the wool, is pumped for re-distribution into tanks until, on obtaining a density of 1·07, it is evaporated and calcined into carbonate of potash. Each machine is capable of dealing with about 8 tons of wool per twenty-four hours. The furnaces consume 1 cwt. of fuel per cwt. of potash salt produced.

The grease is extracted from the suds at the works of Messrs. Alf. Matte and Company, of Roubaix, by a mechanical process of "battage," without the aid of chemicals. The suds are, by means of a rotary-agitator, beaten into a froth, which carries to the surface the insoluble fats. This greasy lather is skimmed off into conduits by a mechanical scraper as it is produced, and is then forced by steam extractors into wooden tanks, in which it is heated to 140° F. Sulphuric acid, in the ratio of 1 lb. to 100 gallons, is then added to clarify it. The acidulated water is decanted and

used to assist in the clarifying, and the magma is passed into open filters of rough canvas. It is then pressed in screw-presses and disposed of as manure, the grease being clarified with sulphuric acid. After the "battage" treatment, the water is allowed to flow into the sewer without further purification.

Paper-Works Waste.—Paper manufactories contribute largely to the pollution of rivers. The effluents are always more or less foul, depending upon the material in hand, and, of course, upon the particular point of time in the washing process, the machines running generally about an hour or two hours. Analyses expressed in parts per 100,000 of the washing-machine waste liquors, taken on the 9th March, 1893, from Messrs. Dimmocks' works, Darwen, are given in the following Table:—

Nature of Sample.	Dissolved Solids.		Total Dissolved Solids.	Suspended Solids.		Total Suspended Solids.	Total Solids.
	Mineral.	Volatile.		Mineral.	Organic.		
Samples from beating- and washing-engines taken every five minutes and mixed	152·2	110·5	262·7	32·1	63·9	96·0	358·7
Samples from breaking- and washing-machines taken every five minutes and washed	18·6	18·3	36·9	8·1	29·8	37·9	74·8

These effluents are, at least in so far as treatment is concerned, subject to the same remarks as bleachers' uncoloured effluent. Upon being mixed with iron sulphate and lime, deposition of solids takes place quickly, and a clear supernatant fluid is obtained. If such treatment and the recovery of soda is adopted, the matter for consideration is reduced to the most economical form of precipitation-tank and filter if necessary.

At Messrs. Peebles' Whiteash Mill, Church, Lancashire, a successful plant is in operation. The whole of the liquors for treatment gravitate to two reception and primary settling-tanks of 9,437 gallons and 9,650 gallons capacity respectively. The overflow from these tanks is conducted to a well, 6 feet deep, having a capacity of 5,000 gallons. From this it is pumped a height of 70 feet to two sets of precipitation-tanks, the first set each 24 feet by 20 feet by 4 feet deep, their combined capacity being 36,000 gallons. The second set is divided into six tanks, each of which is 19 feet 6 inches by 11 feet 5 inches by 5 feet deep, having a

combined capacity of 41,700 gallons. The total capacity, therefore, of the precipitation-tanks is 77,700 gallons, or nearly one day's flow. The tanks are of brickwork with concrete bottoms, and the waste liquors are treated with lime and alumina-ferric as precipitants. After settlement the top liquor is passed through cinder filters, 3 feet deep, of varying grades. The filters are herring-boned with 6-inch drains, and 3-inch branches, the former of which conduct the effluent to the river. On the date of the trial of which the following Table shows the results, in parts per 100,000, the cinder filters had been affected by frost, and were therefore supplemented by a sand filter which relieved them of a large portion of the work. The cinders, too, of the regular filter were fresh from the furnaces, and therefore gave up some soluble matter to the tank effluent, counterbalancing that abstracted.

Nature of Sample.	Dissolved Solids.			Suspended Solids.			Gross Solids.			Remarks.
	Mineral.	Volatile.	Total.	Mineral.	Volatile.	Total.	Volatile.	Mineral.	Total.	
Raw liquor . . .	200	46	246	72	104	176	150	272	422	Turbid.
Tank „ . . .	154	48	202	14	8	22	56	168	224	Fairly clear.
Cinder-filter liquor	154	50	204	14	6	20	56	168	224	Clear.
Sand-filter „	152	44	196	..	..	..	44	152	196	Bright and colourless.

The composition of the mineral solids in each case was:—

—	Silica.	Lime as CaO.	Chlorine.	Sulphuric Anhydride.	Alumina.	Carbonic Acid, Soda, &c.
Raw liquor . . .	24·3	101·3	61·2	51·4	29·3	4·5
Tank „ . . .	1·9	60·3	36·8	54·8	6·8	7·4
Cinder-filter liquor	1·5	42·3	28·3	43·0	4·9	48·0
Sand-filter „	..	28·2	35·0	60 0	..	28·8

The samples were drawn at intervals of two and a half hours. The amount of sludge produced in the precipitation-tanks was 17 tons per day on an average of 28 days, 95·0 per cent. and 98·0 per cent. of moisture being contained in it. This is reduced

by sludge-presses to about 3½ tons per day, the composition of the pressed sludge being—

	Per Cent.
Water	71·50
Sand and silicious matter	5·81
Organic matter	12·74
Lime as CaO	2·45
Alumina as Al_2O_3	2·01
Ferric oxide	0·55
Sulphuric anhydride	0·47
Combined chlorine	0·11
Carbonic acid	2·22
Soda, &c.	2·14
	100·00

As this sludge contains a considerable amount of fibre it is used again, after being washed, for the manufacture of the coarser brown or shop papers. Since the introduction of imported wood pulp as fibre for making paper, the volumes of waste liquors from paper-works generally have been so largely reduced that several attempts have been made to deliver the reduced volume into town sewers. This, however, is undesirable, since the salts of lime, as well as the silicious suspended matter, are liable to be deposited in the form of an incrustation on any small projection in the sewer, and so to eventually block up the sewer, or at any rate to materially diminish its sectional area.

Tannery Waste.—Waste liquors from tanneries form probably the worst manufacturing pollution, containing as they do so much organic putrescent matter which cannot be precipitated by ordinary means, if at all. They are in reality strong sewage, and must be treated as such. So far as the prevention of river-pollution is concerned, such waste is best disposed of by the local authority's sewer. Ratepayers generally, however, are averse to being taxed for the treatment of waste which they do not produce. But in towns where tanning is the staple industry, the community at large should be prepared to make concessions to a trade upon which it exists either directly or indirectly. In other towns, however, where tanning is an isolated industry, reasonable objection is often taken to sewer connections, and in such cases the manufacturers are compelled to adopt the best known means for purifying the waste.

The plant in use at the works of Messrs. W. and J. Sagar, of Colne, forms a good example. It consists of two series of tanks and filters, the first set of tanks receiving the washings of hides,

and refuse from pure shop and bran fermentation. The liquor passes from them, after settlement through fibre screens, to a second series of precipitation-tanks, and is then passed through oxidizing filters to the river. A second series of tanks receives the waste liquors from the lime-pits through the delivery-pipe of a pump; and, after settlement has taken place, it is filtered on specially prepared layers of earth and ashes, formed to contain as much air as possible for oxidation. The effect of this treatment is fairly satisfactory, though it is perhaps not the best process of sewage purification. Various experiments have been made by the Author, the results of which show that the oxidation of organic matter in tannery waste as in sewage is an exceedingly slow biological process which cannot be advantageously hurried. Only a fixed amount can be oxidised by a certain organism under the best conditions. The result of the treatment at Messrs. Sagar's works is generally the total reduction of the suspended solids and about 30 per cent. of the dissolved solids. The albumenoid ammonia in the liquors is also reduced about 40 per cent., but the nature of the raw liquors varies so continually that any figures for less than a month's trial are likely to be misleading.

Alkali-Works Refuse.—By the careful supervision of the inspectors under the Alkali-Works Regulation Act, pollution from this source has now almost ceased. The yellow liquors containing sulphides from the vat waste are treated in most cases by the Chance sulphur-recovery process. But there are many thousands of tons of old vat waste, tipped in various parts of the country, still draining unoxidized sulphides into watercourses. Apart from the Chance sulphur-recovery process, various expedients have been adopted for the treatment of this waste. In a typical plant, the liquors are pumped from subterranean channels and delivered on to a filter of iron oxide. The sulphides are oxidized to a certain extent, and any precipitate formed is caught in two successive precipitation-tanks; while the traces of sulphide still unoxidized are dealt with eventually in the second oxide filter, together with any matter which may remain in suspension after passing through the two precipitation-tanks.

Where large volumes have to be dealt with, so causing the choking of filters, the liquor is treated with a ferric salt which oxidizes the sulphides, and is afterwards filtered through sand or gravel.

Samples taken on the 19th January, 1895, before and after passing through the filter, gave 3·84 parts and 0·64 part per

100,000 of sulphuretted hydrogen respectively. Other samples taken before and after this date gave similar results.

The Galvanizing of Iron.—Before iron sheets will take the zinc from the molten bath, their surfaces must be free from iron oxide and from any adventitious dust and grease. The removal of such matters is accomplished in a bath of sulphuric or hydrochloric acid which gradually becomes saturated with iron salts. Before, however, the point of neutrality is reached, the action of the acid is so slow that it becomes more economical to discard it and to employ fresh rather than to use it further. Local authorities rightly refuse to admit the liquid into common sewers, owing to its action on the cement joints, and to the large amount of lime necessary for the precipitation of the iron at the outfall works. If allowed to pass into rivers directly, or thence through outfall sewers without treatment, it has an injurious effect upon animal and vegetable life, in addition to producing an objectionable deposit of ferric oxide on the banks and beds of rivers.

Where sulphuric acid is used and the waste therefore consists of a solution of ferrous sulphate, a process of neutralisation is adopted, similar to the Weldon process for manganese dioxide recovery; viz., lime is added which forms sulphate of lime with the acid radical, and so sets free iron oxide which in some cases is collected and used as a pigment. Where hydrochloric acid is used and the "waste" is a strong solution of ferric chloride, the Turner recovery process,[1] which has been in successful operation about four years at Messrs. Walker Brothers' galvanizing works, Walsall, gives much better results. The plant consists of a dish-shaped receptacle or pot into which the liquid is run. This is placed before a coke fire, the heat given off gradually concentrating it and causing a continual deposition of the soluble ferric chloride. This is extracted through a hand-hole and is placed in a space between the fire and the dish-shaped receptacle, where it is decomposed by heat into chlorine and ferric oxide. The chlorine passing over the bridge with the draught, comes in contact with the steam rising from the waste, and forms hydrochloric acid, which is conducted to a tower and condensed for use again. The iron oxide is of some value as fettling for puddling-furnaces, yielding a few shillings per ton. The two furnaces at Messrs. Walker Brothers' works together treat 1,000 gallons of waste per day, yielding about 300 to 350 gallons per day of 75 per cent. commercial strength (29° Twaddell). Its

[1] "Industries," vol. x., 1891, p. 163.

150 feet, the greatest depth, more than 60 feet, being in the middle. There had been no protective works to the abutments, such as are usually provided in Indian rivers flowing through alluvium, because the course of the river at this part was considered to be quite steady; but after the experience related it was seen that the usual apron and wings of rubble stone could not safely be omitted. An apron 5 feet thick was therefore provided, as shown in *Fig. 1*, together with curved wings about 500 feet long. The amount of stone thrown round the central piers until the 30th August, 1895, has been 280,000 cubic feet during construction, and 723,000 cubic feet at the cost of maintenance.

The Author, on visiting the bridge in April, 1894, observed that the rail-balk was touching and had split the ballast wall of this abutment. The temperature was high, so that the girder was probably at or near its maximum expansion. It was found also that a portion of the balk had been cut off a few years previously. The only conclusion was, therefore, that the abutment was moving forward. In Sir Bradford Leslie's Paper[1] it is mentioned that a crack had appeared between the abutment and the viaduct; and in view of the former having been sunk 73 feet and the latter only 20 feet into the alluvium, this might have been expected. This crack gradually widened until, in 1890, when the Author first saw the bridge, cracks were produced at the base of the piers. The viaduct had sunk 2 inches and the cracks opened $1\frac{3}{4}$ inch. By testing with a plumb-line in the height available, it was impossible to determine whether the movement had been a tilting one or whether the abutment had moved bodily forward.

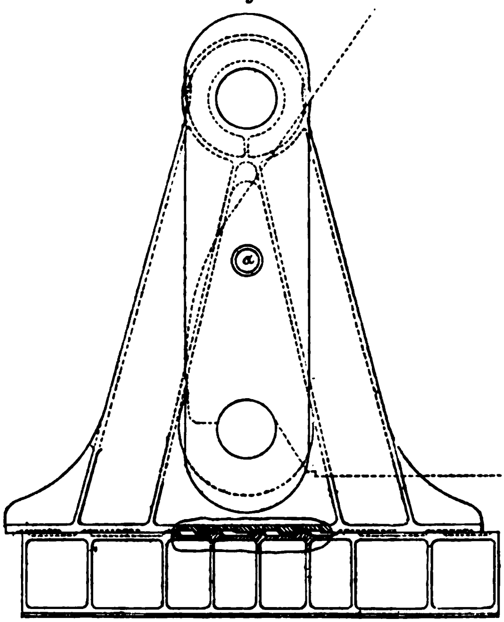
The outside brickwork of the abutment, which to a great extent hid the expansion-gear, having been cleared away, it was found that the superstructure of the abutment was not central with the well, but was built on the forward edge. The pedestal of the link expansion-gear, *Fig. 2*, had also been set back 4 inches on its bed-plate to partly compensate for this shortening of the span, which was found to be still $6\frac{1}{2}$ inches too short, $1\frac{3}{4}$ inch of which was apparently due to the movement of the abutment. This had brought the distance-piece *a* against the back of the pedestal, broken the cast-iron ferrule and bent the pin, which was 3 inches in diameter. The portion in section shows the surface between the pedestal and the bed-plate, made up of chipping-strips 4 inches wide and generally 8 inches apart, so that the setting of the

[1] Minutes of Proceedings Inst. C.E., vol. xcii. p. 84.

pedestal 4 inches back had left it with only the side strip, 1¼ inch wide, for a bearing. The cause of the forward movement of the abutment is therefore apparent. Not only is there an unequal load caused by the superstructure being set forward, but there is a horizontal thrust from the semi-fluid earth behind, loaded with 1·6 ton per square foot by the heavy viaduct.

The most appropriate remedies seemed to be, to build up the back of the abutment with scrap-iron, as near the edge as possible, in order to equalise the pressure on the base, and to increase the

Fig. 2.



EXPANSION-GEAR.
Scale, ¼ inch to 1 foot.

thickness of the stone pitching in front of the abutment to 20 feet, in order to balance the pressure from behind. Further movement will be watched by means of a permanent plumb-bob set inside the abutment at as great as height as possible, and by a vernier scale on the end of the girder marked after repeated observations at dawn for two temperatures, 80° F. and 60° F., so as to obtain a true length for the girder and thus verify its position relatively to the abutment.

The expansion-gear was also repaired without delay, for not

only was there imminent danger of the pedestals being displaced by the girder pressing against them on hot days, but the pedestal, bearing as it was only on the side strip of the bed-plate, could not be considered safe. To place the bearing of the links in their proper positions, the following arrangements were made:—An additional piece 1 inch long was bolted to the back of the bed-plate, and a strong hook was fitted to the lower or girder bearing-pin and connected to the front end of the pedestal by a screw coupling. This was drawn tight in the early morning, and as the girder expanded during the day it dragged its pedestal back until the length of the mean span was nearly correct and the chipping-strips rested in their proper positions on the bed-plate. The downstream pedestal was moved back without difficulty. The upstream pedestal, however, after moving about 3 inches, was stopped by washers and other obstructions in the hollows of the bed-plate becoming jammed between the upper and lower chipping-strips as they approached one another. An eye-bolt was then drilled into each corner of the pedestal to enable it to be lifted by four 10-ton traversing jacks; and, the girder being raised by two 200-ton jacks placed in front of the bed-plate, the pedestal was lifted high enough to clear the obstructions and was then traversed back into its proper position, without stopping the traffic.

This experience at the Jubilee Bridge shows that it is not safe to trust the abutments of bridges in the Indian alluvial plains without rubble-stone protection. Even without a general change in the course of the river, a sudden attack may, as in the present instance, develop with alarming rapidity. It is also evident that even with a well foundation as deep as 80 feet, unbalanced pressure will, in such semi-fluid material, cause movement.

The Author prefers the "stilt" form of roller-gear to the link expansion-gear. The former, if properly constructed, does not give trouble, and in case of any adjustment being required it is more easily handled than the latter.

The Paper is accompanied by two tracings, from which the *Figs.* in the text have been prepared.

(*Paper No. 2921.*)

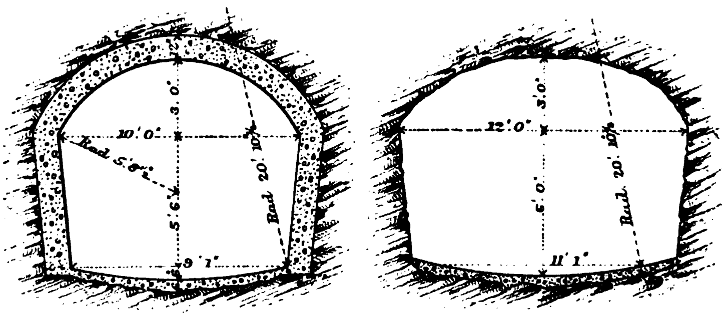
"Observations on the Flow of Water in the New Aqueduct from Loch Katrine: Glasgow Corporation Waterworks."

By Alexander Fairlie Bruce, M. Inst. C.E.

The object of the following notes is to give the results of some observations made by the Author, on the flow of water in the new aqueduct connecting Loch Katrine with Glasgow, with the view of determining the value of the coefficient of friction n, in Kutter's formula,[1] for such channels, and of ascertaining the comparative value of velocities obtained by the use of floats.

For about 53 per cent. of its length the aqueduct is lined with

Fig. 1. *Fig. 2.*



SECTION WITH CONCRETE LINING. UNLINED SECTION.

Scale, $\frac{1}{8}$ inch to 1 foot.

concrete, *Fig. 1*, which was built in the usual way, in 12 or 15 feet lengths, generally the former. Open frames of 6-inch by 2-inch

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$$C = \frac{41\cdot6 + \frac{1\cdot811}{n} + \frac{0\cdot00281}{i}}{1 + \left(41\cdot6 + \frac{0\cdot00281}{i}\right)\frac{n}{\sqrt{m}}}.$$

C = coefficient of discharge.
i = sine of the inclination.
n = coefficient of friction.
m = hydraulic mean radius in feet.

battens were first placed in position, and $\frac{3}{4}$-inch tongued and grooved boards, smeared with soft soap, nailed to them horizontally as the concrete was filled in. Every possible precaution was taken by working with spades, to obtain a good face, and except where some defects showed themselves, no rendering was afterwards necessary. The whole of the invert was concreted, and floated with a steel float, whether lining was required or not, and, as shown in *Fig.* 2, the unlined portions are made 2 feet wider than where lining is required in order to allow for the additional friction. This increases the available area, when the aqueduct is running full (*i.e.*, a foot above the springing level of the arch) from 64·866 square feet to 78·280 square feet, and the hydraulic mean radius from 2·869 feet to 3·106 feet; or 21 per cent. and 8·25 per cent. respectively. These increased values were fixed upon by Mr. J. M. Gale, in designing the works, on the basis of the values of n, found by him experimentally for the lined and unlined portions of the old Loch Katrine aqueduct, which proved to be 0·0184 and 0·025 respectively.

The part of the aqueduct selected for experiment is perfectly straight, extending from the access chamber at High Lettre to that at the Blairgar aqueduct bridge, a distance of 1,921 yards. Rather more than half of it is lined, and the gradient is 1 in 5,500. The depth of water flowing in the aqueduct was measured on the gauge-rods in the chambers, and the true mean velocity was arrived at by dividing the quantity of water measured over the gauge-weir at Mugdock reservoir[1] by the area of flow in the aqueduct. The results are given in Table I in the Appendix.

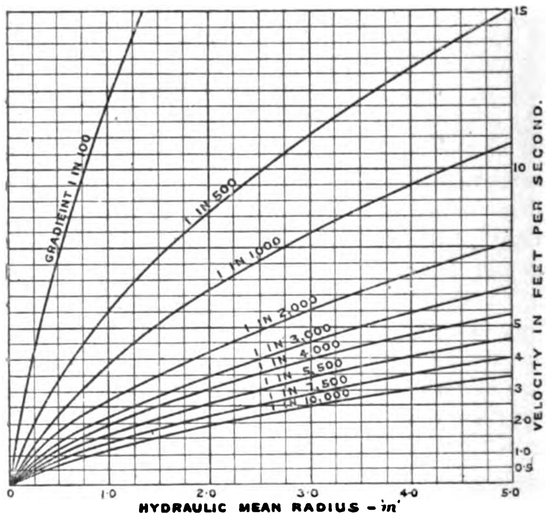
For comparison with the above experiments surface-floats were used, made of circular pieces of wood $1\frac{3}{4}$ inch diameter, and $\frac{3}{4}$ inch thick, and rod-floats 1 inch diameter, and 2 feet 2 inches long, weighted with lead so as to be immersed to the extent of 1 foot $9\frac{1}{2}$ inches, and painted white; hooks were screwed into the top to enable them to be more easily recovered, and each set of floats was numbered from 1 to 10. The floats were placed in the water, at intervals of between 30 seconds and 45 seconds in the middle of the chamber, and timed as they entered the tunnel. Five or six of each series were usually between 1 minute and 2 minutes slower than the mean, owing, probably, to their having been caught and detained in eddies in the unlined portion. These were

[1] Calculated according to the formula: $Q = 3{\cdot}421\ l\ \sqrt{h^3}$; where Q is the quantity of water discharged in cubic feet per second, l is the length of the weir in feet, and h the head above its lip in feet.

rejected, and the results given in Table I are the means of the four or five remaining, which usually came within thirty seconds of each other.

It would appear from these experiments, that velocities obtained by the use of the surface-floats are, in such cases, unreliable; those in the present instance giving a result, if Darcy's formula[1] be applied to calculate the mean from the surface velocity, about 18 per cent. less than the correct figure. On the other hand, though the velocities obtained by the rod-floats fluctuated somewhat, they gave a mean result of only about $\frac{3}{4}$ per cent. less than the true mean, which is a sufficiently close approximation for most purposes.

Fig. 3.



The values deduced for n vary between 0·0119 and 0·0129, the mean being 0·0124, except in one or two cases, where, perhaps, there was some slight uncertainty in reading the quantity flowing over the gauge-weir, the variation being generally only about 0·0001 on either side. This value is half-way between those given by Kutter, for "unplaned planks," and "ashlar and brickwork," and agrees with Mr. W. J. B. Clerke's figures, for the

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[1] $$v_m = v_s - 25\cdot 4 \sqrt{m} \times i.$$

v_m = mean velocity in feet per second.
v_s = surface " "

aqueduct in connection with the Tansa Waterworks for the supply of Bombay.[1] The large value obtained by Mr. Gale, in his experiments on the lined portion of the old aqueduct, is no doubt due to its being lined with masonry, the joints of which are very open, all the mortar having been washed out of them. This causes the formation of eddies, thereby greatly increasing the surface friction. The velocities of discharge have been calculated, for various values of m and i when $n = 0{\cdot}0125$, and the velocity curves, *Fig. 3*, have been plotted from them.

Some observations on the discharge of the siphon pipes across the Blane Valley are given in Table II in the Appendix. There are two lines of 48-inch pipes, 3,557 feet long, which, at the date of the experiments, had been laid between two and three years, and charged with water four months, so that they had been exposed to a certain amount of corrosion. The volume discharged was measured at the gauge basin, as in the case of the aqueduct experiments, and the levels of the water, both at the valve chamber where it enters the pipe, and at the basin where they deliver it again into the aqueduct, were carefully measured. The quantity delivered by the pipes for various hydraulic gradients is given in Table II, and is practically identical with that obtained by calculation, using Professor Unwin's modification of Darcy's formula, for new pipes coated with pitch;[2] viz., $v = 69\sqrt{di}$, where v is the velocity of the water in feet per second, d the diameter of the pipe in feet, and i the hydraulic gradient.

In conclusion the Author desires to express his indebtedness to Mr. Gale, Chief Engineer to the Glasgow Corporation Waterworks, for the facilities granted him for making the above experiments.

The Paper is accompanied by two tracings, from which the *Figs.* have been prepared.

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[1] Minutes of Proceedings Inst. C.E., vol. cxv. p. 27.
[2] Encyclopædia Britannica—Hydraulics, p. 485.

[APPENDIX.

APPENDIX.

TABLE I.—OBSERVATIONS ON THE FLOW OF WATER IN THE AQUEDUCT AT DIFFERENT LEVELS.

Depth of Water in Aqueduct.		Cross-Sectional Area of Stream.	Discharge over Gauge Weir.	Mean Velocity.	Hydraulic Mean Radius, *m*.	Coefficient of Discharge, C.	Coefficient of Friction, *n*.	Surface Velocity by Floats.	Mean Velocity by Weighted Rods.
Ft.	Ins.	Square Feet.	Cubic Feet per Second.	Feet per Second.	Feet.			Feet per Second.	Feet per Second.
1	$8\frac{5}{8}$	14·225	26·620	1·8714	1·2273	125·35	0·0123	2·1139	
2	$1\frac{7}{8}$	18·299	37·863	2·0691	1·4713	126·58	0·0125		
2	$1\frac{7}{8}$	18·299	38·567	2·1076	1·4713	128·93	0·0123		
2	$2\frac{3}{8}$	18·692	41·423	2·2161	1·4896	134·59	0·0119		
2	$2\frac{1}{2}$	18·788	39·986	2·1283	1·4982	128·88	0·0124		
2	$2\frac{5}{8}$	18·888	40·702	2·1549	1·5002	130·41	0·0123		
2	$3\frac{3}{4}$	19·765	42·890	2·1750	1·5469	129·62	0·0124	2·1836	2·1631
2	5	20·774	45·832	2·2062	1·5990	129·33	0·0125	2·2600	2·1986
2	5	20·774	45·832	2·2062	1·5990	129·33	0·0125	2·2554	2·2309
2	5	20·774	45·832	2·2062	1·5990	129·33	0·0125	2·2422	2·2095
2	5	20·774	45·832	2·2062	1·5990	129·33	0·0125	2·2621	2·2020
2	5	20·774	45·832	2·2062	1·5990	129·33	0·0125	..	2·1970
2	$5\frac{1}{8}$	20·857	46·582	2·2334	1·6071	130·58	0·0125	2·2571	
2	$5\frac{1}{4}$	20·931	46·582	2·2255	1·6105	130·14	0·0125	2·2340	
2	$5\frac{1}{2}$	21·132	47·335	2·2451	1·6208	130·72	0·0124	2·2275	2·1911
2	$5\frac{3}{4}$	21·330	48·092	2·2547	1·6270	131·02	0·0124	2·2466	2·1680
2	$8\frac{3}{4}$	23·687	53·504	2·2588	1·7403	126·92	0·0129	2·2335	2·2215
2	$11\frac{1}{4}$[1]	25·627	53·504	2·0845	1·8122	135·58	0·0121	2·1271	2·1054

[1] The sluice on one of the Blane siphon pipes was closed, so the water was dammed back to a surface slope of 1 in 7,711.

TABLE II.—DISCHARGE OF 48-INCH PIPES FOR VARYING HYDRAULIC GRADIENTS.

Ruling Gradient.	Discharge of Pipe Measured at Gauge Weir.	Velocity.	Discharge Calculated by Formula.	Velocity by Calculation.
	Cubic Feet per Second.	Feet per Second.	Cubic Feet per Second.	Feet per Second.
1 in 5,928	1,352·6	1·798	1,347·6	1·787
„ 5,081	1,437·6	1·908	1,455·7	1·930
„ 3,785	1,628·9	2·161	1,686·6	2·237
„ 1,423	2,750·0	3·649	2,750·9	3·648

(*Paper No. 2922.*)

"The North Sea and Baltic Canal."[1]

By MAX AM ENDE, M. Inst. C.E.

Numerous schemes for a connection between the mouth of the River Elbe in the North Sea and the Baltic Sea, to avoid the perilous passage round the north coast of Denmark, have been suggested since the end of the 14th century. The Stecknitz Canal, planned by the city of Lübeck for uniting the Trave, flowing into the Baltic at Lübeck, with the Delvenau, flowing into the Elbe at Lauenburg, was carried out in 1391–1398, and is still in existence. A canal between Lübeck and Hamburg, using the rivers Trave and Best on one side and the Alster on the other, was constructed in 1525 by the two cities, but was abandoned in 1550. Both canals were of insufficient dimensions for the use of sea-going vessels even at that time. About the end of the 16th century a larger canal was proposed to the Emperor Maximilian II. of Germany, and other projects were put forward in the 17th century under the auspices of Wallenstein, Cromwell, the Dutch Republic and the city of Lübeck. These were succeeded by various plans, until, in the reign of Christian VII. of Denmark, between 1777 and 1784, a canal for sea-going vessels, of a depth of 10 feet, was made between Holtenau on the Baltic and Tönning on the North Sea, using chiefly the course of the Eider, which flows into the North Sea at that place. Between 1848 and 1863 the question of coast defence entered into consideration for a canal of larger dimensions. The Prussian navy had come into existence and the harbour works at Wilhelmshafen were progressing. A design contemplating a canal between Kiel and Brunsbüttel, somewhat to the south of the present line, was worked out in great detail. Between 1864 and 1870 Count Itzenplitz, Prince Bismarck, von Roon, Count Moltke, and lastly King William I. became interested in the subject, but the war of 1870 delayed further action.

After the war, when the financial position of the German Empire

[1] A list of works relating to the Canal is given in the Appendix.

was favourable, the canal was again discussed as a matter of national importance; but the financial depression which followed, and in a large measure Count Moltke's publicly expressed want of confidence in the utility of the canal for military purposes, had the effect of silencing, for several years, all voices in its favour. A pamphlet by Mr. Dahlström, a merchant of Hamburg, showing the probable remunerative character of the canal, in 1878, however infused fresh life into the matter. Eventually, through his perseverance and through the influence of Prince Bismarck and the Emperor William I., the line and dimensions of the canal having been determined, mainly according to the plans of Mr. Lentze, and the cost having been estimated at £7,800,000, it was decided by Act of Parliament in 1886 that the German Empire should furnish £5,300,000 and Prussia £2,500,000, besides its share in the Imperial contribution.

The estimate consisted of the following items:—

	£
Purchase of land and compensation	495,000
Excavation and dredging	3,545,000
Consolidation of banks and slopes and demarcation of channels in the lakes	360,000
Harbours, quays, locks, &c.	1,812,500
Bridges and ferries	335,000
Fortifications	50,000
Buildings	65,000
Installations for working the canal	112,500
General expenses	1,025,000
	£7,800,000

It was further decided that the execution of the work should be conducted by the Imperial Home Office, the plans having until this time been under the revision of Mr. Baensch of the Prussian Public Works Office. A commission of eighty-six Prussian and other German Government Engineers was constituted at Kiel, under the presidency of Messrs. Loewe and Fülscher, on October 1st, 1886, and the foundation stone of the lock at Holtenau was laid by the Emperor William I. on June 3rd, 1887. On June 22nd, 1895, the canal was opened for traffic by his grandson, the Emperor William II., the navies of Europe and that of the United States being represented by sixty ships.

From its commencement, at a point near Brunsbüttel, where the River Elbe has a westerly course, the canal runs in a north-easterly direction. Vessels enter it between two stone piers forming the funnel-shaped outer harbour, and pass through one chamber of the double lock into the inner harbour and the canal itself. At the other end they pass through a similar harbour and lock, and

enter the open water of the Bay of Kiel in an easterly direction after a journey of 98·65 kilometres (61·31 miles). The Elbe Marshes extend for 12 miles from the commencement of the canal, which passes, at 5 miles, through the Kuden Lake and at between 9 miles and 10 miles, in a more northerly direction, the moor near Burg. At Grünenthal (19 miles), where the watershed between the Rivers Elbe and Eider is reached, the direction being again more easterly, the canal enters the valleys of the Gieselau and Eider, crosses the Reit Moor, passes through the Meckel Lake (30 miles) and reaches the River Eider at Schülp (34½ miles), from which it is separated for the next 2½ miles by a dike. The canal then turns by a double curve to the south of the old town of Rendsburg, where a lock connects it with the Eider, and enters the upper Eider Lakes at Audorf (40 miles). Beyond 45 miles it lies to the north of the Eider Canal, but joins it again at 50 miles, passes the Flemhude Lake, crosses the watershed between the two seas through cuttings, and terminates in the inner harbour of the Holtenau Lock at 60 miles.

The mean water-level of the canal is the same as that of Kiel Harbour. In the Baltic the highest water-level is 10·4 feet above, and the lowest 6·86 feet below that in the canal; in the Elbe, ordinary tides rise 4·99 feet above and fall 4·17 feet below it, and spring tides rise 17·19 feet above and fall 10·4 feet below it. The depth of the canal at mean water-level is 29·5 feet between Holtenau and Rendsburg, increasing to 33·47 feet at the Elbe Lock. Accordingly the canal is mainly fed by water from the Baltic, but it also derives part from the inland watercourses. As the ratio of salt to fresh water is 9 to 1, it is expected that the water in the canal will not freeze in winter oftener than that in Kiel Harbour or at the mouth of the Elbe, events which rarely happen. Of the whole length of 61·31 miles, 38·62 miles of the canal are straight. The curves vary between 6000-metre and 1000-metre radius. The width of the horizontal bed of the canal is 72·18 feet; this is increased by amounts varying between 1 metre on curves of less than 2,500 metres and 16 metres on those of 1000-metre radius. On each side of the locks the width of the bed is 244·5 feet, and at the six passing-places, which are about 7½ miles apart, and each about 1,476 feet long, the width of the bed is 196·8 feet. In the Eider Lake at Audorf the width is sufficient for the largest vessels to turn. The sides of the canal have a slope of 3 to 1 up to a height of 9·84 feet and then of 2 to 1 to a height of 22·97 feet above the bed. At this level, 6·56 feet below mean water-level, the width being 183·7 feet, a berm is constructed, 18 feet wide in the low lands

and 8·2 feet in the higher territory, the greater width being provided with a view to future widening of the canal. Above the berm the sides have a slope of 1½ to 1 protected by stone to 2·3 feet above mean water-level. At this point a second berm occurs, 8·2 feet wide, above which the dikes, 6·56 feet wide at the top, rise to 6·33 feet above mean water-level. The dikes generally have slopes of 2 to 1, but where the soil is soft the inner slope is 6 to 1.

The total quantity of soil removed in the construction of the canal was 106,000,000 cubic yards. About two-thirds of this, mainly consisting of boulder-clay with layers of liquid peat and sand, could be excavated in the dry, and be used partly for raising the tracts of land by the side of the canal, and partly for filling the lakes. About one-third was boggy soil and had to be dredged, a portion of this material being carried into the open sea. So little consistency had this soil that, for example, in the Meckel Moor, it was carried behind the dikes in a line of floating pipes 2,000 feet long. The number of excavators employed was twenty, of dredgers and elevators forty-six, and of steamers and vessels of all kinds two hundred and forty-seven. There were also ninety-four locomotive engines and 2,756 trucks at work. Over short distances in the Reit Moor, the Meckel Moor and near Schested (45 miles), but particularly over a distance of 7 miles in the low lands of the Kuden Lake near Burg, the excavations were attended with considerable difficulty. Here the sandy soil, the former bed of the sea, or clay with sand, lies at a depth of 26 feet to 33 feet below the surface, and the bed of the canal is therefore on firm ground. Above this, however, the clay is soft, and is overlaid by a bed of marshy soil, floating sand and sedge peat between 16 feet and 26 feet thick. This material was too soft to form the banks of the canal, which had therefore to be made of sand brought from the rear. The first 3 miles or 4 miles from Brunsbüttel were excavated in the dry. Beyond that the work began with making the dikes of the canal by depositing the clay excavated in the rear. These dikes sank deeper the more nearly the Kuden Lake was approached, and slips occurred when the soil was excavated between them. Sand was then resorted to in place of the clay. Where the ground was still firm enough to support a 2-foot gauge railway and trucks holding 17 cubic feet of sand, a mattress of sand, about 3 feet thick and 50 feet broad, was laid by them after the turf, 1½ foot thick, had been removed from the surface. In the rear a 3-foot gauge railway followed with trucks holding 106 cubic feet of sand. As the sand was deposited on the mattress,

it gradually sank, assuming an irregularly rounded profile of about 50 feet in width or more, to the firm ground, heaving up the soft ground on both sides. Where the ground was too soft to support even the 2-foot gauge railway, the latter had to be projected on piles or, as in the Kuden Lake, on floats, and the sand forming the mattress was tipped over from the small trucks, the heavy trucks following for making the dams. This plan of making sand dams on both sides proved perfectly successful, as between them the profile of the canal could be cut out by the dredgers without further difficulty. Between points 5 miles and 8·2 miles distant from Brunsbüttel 772,000 cubic yards of sand were deposited in this way, and between 8·2 miles and 11·2 miles 1,590,000 cubic yards.

At Holtenau, as well as at Brunsbüttel, there are two locks side by side as already mentioned, separated by a platform 41 feet wide. Each has two pairs of mitre gates at each end, one for inside and the other for outside pressure. In the middle also there are two pairs of gates of light construction, but with large oak shutters. These gates are only closed if the current in the lock is too strong for conveniently closing the main gates. Besides these gates there is at each end of each lock a caisson which can be let into a groove in the masonry in the event of repairs being necessary. Each lock has four culverts 13·32 feet by 6·21 feet, from each of which six culverts 4·1 feet by 3·28 feet branch off into the lock. The available length of each lock is 492·14 feet, and the width 82 feet. The total length of the masonry containing the two locks is 712 feet, and the width 269 feet. This rests on a bed of concrete 9·84 feet thick at Holtenau, and 11·5 feet at Brunsbüttel. The excavation for the Baltic lock was effected in the dry, the accumulating water being removed by powerful pumps. In the marshy soil at the Elbe lock, the water-pressure being too great, the soil was dredged out, and the concrete forming the 11½-foot bed had to be lowered into the water. When it had hardened the whole was enclosed by cofferdams, and the water could then be pumped out.

The quay-walls of the inner harbour of the Elbe lock rest on piles driven into the marshy soil. When, after their completion, the soil between them was excavated, one of them yielded to the outside pressure, but was soon repaired. This, with the exception of a few slight slips, was the only mishap occurring in the construction of the canal. The working of the gates is by hydraulic power from the middle platform. The Elbe lock is generally open during the last half of ebb tide, and closed when the water rises above mean level. The Baltic lock is always open, except

when the level of the sea is more than 1·64 foot above or below mean level, a condition which obtains during about twenty-five days in the year.

There are two road-bridges, five railway-bridges, and thirteen wire-rope ferries crossing the canal. The road-bridge at Holtenau is carried on double pontoons. That at Rendsburg, sustaining the road to Itzehoe, has a hydraulic swing-span of 164 feet. The two railway swing-bridges at Rendsburg, of 164 feet span, bear each one line of the Rendsburg-Neumünster Railway; and that at Taterpfahl, 3 miles from Brunsbüttel, the single line of the Elmshorn-Hvidding Railway. The West Holstein single-line railway and a road cross the canal at Grünenthal (18·6 miles) on an arch bridge of 513·4 feet span and 137·8 feet height. The width of 21·3 feet is used both by trains and road vehicles, the trains being signalled for clearing the bridge. The footways outside the arches rest on cantilevers. The Kiel-Flensburg single-line railway and the Kiel-Eckernförde road cross at Levensau (about 4 miles from Holtenau) on an arch bridge of 536·1 feet span and 137·8 feet clear height. The original estimate provided for a swing-bridge at this place costing £160,000 less; but as there was an adequate saving in the cost of the canal, the fixed high-level bridge was decided upon, the balance being sufficient to cover the additional expenditure thereby involved.

The number of workmen employed during the seven years occupied in the construction of the canal varied between 3,000 and 8,000. Most of them were housed and boarded in barracks built and managed by the Canal Commission. The dormitories generally contained one hundred beds in fourteen rooms; breakfast and dinner were taken in a common room, the cost of lodging, lighting, warming and cleaning the rooms being eightpence per day per man. Articles of food, tobacco and beer were also sold by the manager at fixed prices in the canteens. The arrangements for board and lodging of artizans were of a somewhat superior and less regular kind, and were generally carried out by contract under the supervision of the Commission. For the care of the sick arrangements were made with the municipalities of the towns; but for light cases rooms were set aside in every barrack, and a surgery was provided, which were visited at regular intervals by an inspecting medical officer. In addition hospitals were erected at Burg and Hanerau by the Commission. In 1892, when the cholera raged at Hamburg, the Commission erected special hospitals at five points, and the workmen were instructed in methods of healthy life, with the

result that only a few cases of cholera occurred among them. Although marsh fever had been anticipated, neither this nor cases of influenza were noteworthy.

The success of the canal as an engineering undertaking and in respect of economy in execution is unquestionable. Inasmuch also as it connects the two most important naval stations of Germany, Kiel harbour on the Baltic, and Wilhelmshafen on the North Sea by a protected route, no doubt can be raised as to its importance to the German Empire. The entrance from the Bay of Kiel into the canal lies 2 miles south of Friedrichsort, a point where the strongly fortified coasts of the bay are only 2,600 feet from each other. At the other end the open sea, where a hostile fleet is not prevented from manœuvring by sandbanks, lies at a distance of about 30 miles from the entrance; while the batteries on the island of Neuwerk will protect the passage of vessels turning the corner at Cuxhaven, in their course from Brunsbüttel to Wilhelmshafen, as the island lies northward of that corner, and about 22 miles from the entrance of the canal. The canal is further important as a means of avoiding the perilous navigation through the Skager Rack, where about two hundred vessels were lost annually in the years 1877 to 1881, of which forty were German.

More uncertain is the question whether mercantile vessels will take advantage of the shortening of their voyages to the extent of rendering the canal remunerative. Vessels from the east join their courses at a point south of Copenhagen in Lat. 55°. With this as a starting point, and travelling at a speed of 8·25 knots, vessels from Hamburg, Bremerhafen, Emden, Rotterdam, Newcastle, London, Leith and Dunkirk would save 44·91, 32·54, 27·69, 22·11, 6·36, 22·36, 3·57 and 22·35 hours respectively. According to Mr. Dahlström, the cost of coal, wages and board, assurance, &c., amount, for a cargo-steamer of 800 registered tons, to between £30 and £40 per day, for a coal-steamer of equal size to between £18 and £20, and for a sailing vessel to £5 or £6. In the estimate of remunerability made for the Canal Bill of 1886, it was assumed that of the 35,000 vessels passing annually through the Sound, near Copenhagen, 18,000 vessels, with a total registered tonnage of 5,500,000, would avail themselves of the canal. This would, at a mean charge of 9*d.* per ton for the canal passage, represent a net income of £111,200, equal to about 4 per cent. interest on £2,300,000, so that about £5,000,000 would have to be found without interest. To that estimate of the number of vessels passing the canal, Mr. Dahlström adds 2,258 vessels which used the Eider canal for longer sea-voyages, and 1,000 to 1,500

vessels for local traffic, as also 8,000 vessels passing through the two Belts. During the years 1880 to 1889, the gross tonnage of vessels passing through the Sound has increased 26 per cent. The dues actually levied since the opening of the canal are somewhat lower than assumed in 1886, namely for loaded vessels 7·2*d.* per ton up to 600 registered tons, and 4·8*d.* for every additional ton. Vessels laden with ballast and coasting vessels of less than 50 tons are charged 4·8*d.* per ton. Towage of sailing vessels by the regulation tugs is charged 4·8*d.* extra up to 200 registered tons, and 3·6*d.* for every additional ton. In the case of unloaded vessels this is reduced to 3*d.* and 2·4*d.* respectively. During the six months between October and March, the dues will be increased by 25 per cent. The present arrangements will be tried for about one year, when the settlement of the dues will be submitted to the Reichstag.

APPENDIX.

List of Works relating to the North Sea and Baltic Canal.

Lentze, "Denkschrift über den Entwurf zum Bau eines Schiffs-Kanals zur Verbindung der Ostsee mit der Nordsee, bearbeitet im Auftrage des König. Ministeriums fur Handel, Gewerbe und öffentliche Arbeiten, Berlin, 1865.

H. Dahlström, "Die Ertragsfähigkeit eines Schleswig-holsteinischen Schiffahrt-Kanals," Hamburg, 1878.

Sympher, "Der Nord-Ostsee-Canal," Centralblatt der Bauverwaltung, 1886, p. 233.

Baensch, "Der Nord-Ostsee-Canal," *ibid*, 1889, pp. 73, 84, 92.

Baensch, "Vom Bau des Nord-Ostsee-Canals," *ibid*, 1891, pp. 193, 203, 214.

Koch, "Die Hochbrücke über den Nord-Ostsee-Canal bei Levensau," *ibid*, 1894, p. 508.

"Die künftige Betriebsverwaltung des Nord-Ostsee-Canals," *ibid*, 1895, p. 217.

Sympher, "Die Vollendung des Nord-Ostsee-Canals," *ibid*, 1895, p. 265.

C. Beseke, "Der Nord-Ostsee-Canal," Kiel und Leipzig, 1893.

J. Fülscher, "The North and East Sea Canal," prepared for the International Engineering Congress of the Columbian Exposition, 1893. Translated for the Transactions of the American Society of Civil Engineers (vol. xxx., 1893, p. 421) by Kenneth Allen, M. Am. Soc. C.E.

J. Fleury, "Le Canal de la Baltique à la Mer du Nord. Mémoires de la Société des Ingénieurs Civils," 1893, i., p. 717.

von Kuntze, "Der Nord-Ostsee-Kanal." Zeitschrift des Vereines Deutscher Ingenieure, xxxviii., 1894, p. 1220.

C. Loewe, "Geschichte des Nord-Ostsee-Kanals," Berlin, June, 1895.

Count E. von Dürkheim-Montmartin, "Der Nord-Ostsee-Canal." Mittheilungen aus dem Gebiete des Seewesens, xxiii., p. 697. Pola, 1895.

"The North-East Sea Canal," *Engineering*, May to September, 1895.

OBITUARY.

PETER DENNY, LL.D., was born at Dumbarton on the 31st of October, 1821. His father, William Denny, was a ship-builder of that town, and came of a family of farmers who, for several generations, had been established at the entrance of the Vale of Leven. In the year 1833, when Peter Denny was only twelve years old, his father died, leaving a widow with eleven children, of whom four were too young to be of any assistance to her in the impoverished condition in which she found herself. The lad remained at school in Dumbarton until his fourteenth year, when he obtained employment, first as a clerk to the local sheriff's officer, and afterwards at Christie's Glass Works, then the staple industry of the town of Dumbarton. He remained as a clerk in the employment of the Messrs. Christie until May, 1843, when, through some misunderstanding, he left their service and became a pupil of his elder brother William, who was at that time managing Messrs. Napier's shipyard at Govan. Very shortly afterwards William went to America, and Peter joined his brother Alexander, then doing a good business as a naval architect in Paisley.

At this stage it becomes necessary to follow the movements of the other members of the family; for the evolution of Peter Denny as a notable shipbuilder is inextricably entwined with the careers of his brothers William, James, Alexander and Archibald. The premises occupied by their father were known as the Wood-yard, and were situated on the west side of the River Leven, not far below the bridge. In that yard William Denny, senior, had first worked as a carpenter, then managed as a partner, and ultimately controlled as proprietor. The class of ship-work then done on the Leven was, however, of a very humble character, consisting, for the most part, of small wooden coasting smacks, schooners, and the like. Such as it was, it seems to have been well done, and the yard was provided with one of Morton's patent slipways, which was an indication of enterprise and advancement in those days. His eldest son, John, assisted him in the business, while

James, Robert, William, Alexander and Archibald learnt their trade as carpenters under their father at the old Woodyard. On the death of his father, John Denny entered upon the tenancy of the shipyard, while James went to America, where he remained until the year 1847. Robert Denny became a ship carpenter, and both William and Alexander moved about from place to place, wherever they could obtain employment.

Of this family of shipwrights William Denny was, without doubt, the most able. He was early recognised as a superior tradesman and a skilful naval architect. It was in this latter capacity that he chiefly distinguished himself, and it was after being in business as such for several years, that he became manager of Messrs. Napier's shipbuilding yard at Govan, leaving his private business as a naval architect to his brother Alexander, who also had ere this shown ability in the same direction. It was while William Denny was with Messrs. Napier that Peter Denny left the Dumbarton Glass Works. Writing of this incident, Peter Denny tells the story thus:—

> "My brother William kindly asked me to come and stay with him in Glasgow and he would make a ship-builder of me, setting me at once to work on plans at his lodgings. I felt it so difficult and strange that I would have given it up at any moment, but with his usual kindness he had patience with me and persuaded me to stick to it. I did, and soon got confidence and progressed so that he was able to take me in as an assistant in the Govan Yard, I think towards the end of the year, August, 1843, at thirty shillings a week."

But the duration of William's subsequent stay with Messrs. Napier was very brief, as he went to America at the close of the year 1843. Peter Denny then joined his brother Alexander at Paisley, where he was able to be of considerable use in preparing specifications, plans, &c. William returned from America in 1844, when he, Alexander and Peter formed a partnership as naval architects under the name of Denny Brothers, and found plenty of employment. Towards the end of the year, however, they determined to begin business as shipbuilders, commencing operations in January, 1845, upon the east shore of the River Leven just below the parish churchyard. Their elder brother, John, had now been dead many years, and the old Woodyard was occupied by strangers.

Up to this period only wooden ships had been built on the Leven, these being for the most part small craft ranging from about 50 tons to 200 tons, and the largest a barque of 603 tons. Besides the business carried on at the Woodyard, Messrs. McMillan were building vessels of wood at the Dockyard,

on the east side of the Leven near the parish church; so that when Denny Brothers started, there were three shipyards in the town of Dumbarton. But from the first the Denny's used iron, commencing with the paddle-steamer "Lochlomond" of 95 tons and 70 nominal HP. In their first year they built also the iron paddle-steamer "Rob Roy," of 30 tons and 15 HP., and the screw-steamer "Water Witch," of 240 tons and 35 HP. This latter vessel, destined for the Dublin and English trade, was the first mercantile screw-steamer built on the Clyde. The reasons for the preference shown by the young firm for iron were probably two-fold. In the first place, William and Alexander Denny had, for several years previously, designed and superintended the construction of nearly all the iron vessels built on the Clyde; and in the second place, the intelligence and foresight of the brothers convinced them that the days of wood as a material for ships were fast coming to a close. To them it was evident that, with the advent of steam navigation, the iron ship must become a necessity, if the capabilities of the marine engine were to have full scope for development. It was this clear vision and faith in the future of steam-ship building which made the reputation of the firm of Denny Brothers, and led to the unique and honoured position among British shipbuilders and engineers occupied by Peter Denny for many years before his death. The firm stuck to steam with a tenacious grip, and in the whole course of its career, during which time it has built nearly 530 vessels, it has produced only four sailing ships.

Not having workshops for the manufacture of marine engines, the young firm had for five years to get its engines from sub-contractors. Upwards of twenty vessels were, however, built by the firm before Dumbarton had a marine-engine works of its own. In the year 1846 James Denny returned from America and joined them, and three years later, the firm of Denny Brothers was dissolved by mutual consent; William, James, and Peter paying out Alexander and starting afresh under the name of William Denny and Brothers, the designation retained to this day. Alexander started building in a small yard on the north side of the town, taking with him his brother Archibald, under the name of Alexander Denny and Brothers. In 1847 the firm of William Denny and Brothers obtained possession of the old Woodyard, and shortly afterwards the premises near the parish church were taken over by Alexander Denny and Brothers. It is interesting to note that the "Lochfyne," a screw-steamer of 83 tons, built by William Denny and Brothers in that year, is still employed in the daily

carriage of goods between Dumbarton and Glasgow. Up to the close of the year 1851, the firm had built thirty-one vessels, of which twelve were paddle-steamers, sixteen were screw-steamers, and three were sailing vessels. The largest of the steamers was the "British Queen," of 772 tons and 150 HP., and the largest of the sailing vessels was the "Three Bells," which afterwards distinguished herself through standing by and rescuing the whole of the crew of an American ship-of-war, which foundered in Mid-Atlantic.

It became apparent to Peter Denny about the year 1850 that the steam machinery of the firm should be made in Dumbarton, and he therefore proposed to his partners and brothers—William and James—that he should commence an engineering business in the town. This was agreed to, and he entered into partnership with his friend, Mr. Tulloch, of Greenock, and his brother-in-law, Mr. John MacAusland, the firm commencing as engineers in Dumbarton, under the title of Tulloch and Denny, which in 1862 was changed to Denny and Co., the designation still retained.

Having now an engineering establishment partly controlled by one of themselves, Messrs. William Denny and Brothers in the year 1852 came to the front as builders of large iron steamers. Until that time the fleets of the principal ocean passenger lines were, for the most part, composed of wooden vessels and propelled by paddles. The reputation of William Denny and Brothers was such as to induce the Cunard Company to intrust them with the hull and engines of the iron screw-steamers "Australian" and "Sydney," each of 1400 tons and 300 nominal HP. These two vessels, when completed, were, however, at once sold for the Australian Mail Service; but in the same year the "Alps" and "Andes," each of 1440 tons and 300 HP., the first screw-steamers employed in the transatlantic passenger trade, were built by the firm for the Cunard Company. The engines of these vessels made by Messrs. Tulloch and Denny, were of the "beam geared type," with 66-inch cylinder and 54-inch stroke. The steam-pressure used in those days was about 6 lbs. per square inch, and the coal consumption 5 lbs. per I.HP. per hour.

By the close of the year 1853 Peter Denny and his partners had built forty-three vessels; of which forty were steamers and twelve had been engined by Tulloch and Denny.

It was in the year 1854 that, in the words of Peter Denny—

"A great calamity to me took place in the death of my brother William, aged 39. I was so depressed and disheartened that it needed all the encourage-

ment of my friends in consenting to carry on the business, being so imperfectly qualified as to the technical and practical work, and having the necessity of the commercial department wholly upon myself."

But it was the habit of Peter Denny to set up few claims on his own account for the success of the firm. He was aided by excellent managers, of whom not the least able were Mr. Walter Brock and Mr. John Ward, now partners in the firm. It is also a matter of history that to his son William, who died in 1887, the firm owes a very large share indeed of its pre-eminence among shipbuilding establishments. But the personality of Peter Denny is a factor in the commercial development of Dumbarton and its shipping industries, the importance of which it is perhaps yet too early to correctly estimate.

James Denny ceased to be a partner in May 1862, and for some years afterwards Peter Denny controlled the entire business.

The engineering firm of Tulloch and Denny became known as Denny and Co. in the same year. Their original shop still exists and constitutes about 6 per cent. of the total area now covered by their engineering premises. Peter Denny states in his Presidential Address to the Institute of Marine Engineers, delivered in 1891, that from 1845 to 1860 no material advance was made in marine engineering. "Pressures were gradually increased with a larger range of expansion in the cylinders, and at one time it was thought that superheated steam was to revolutionise everything. Then surface condensers replaced the old jet ones, but the real advance only came with the compound engine."[1] He made no claim for himself or his firm in regard to the early introduction of the compound engine in ships. On the contrary, he has left it on record that about the year 1867, when the compound system began to attract attention, they were often asked by their customers in regard to it, but could give little information, because they knew little and could not obtain trustworthy particulars. On asking his old friend Mr. Randolph, of Randolph and Elder, for help, he was told to find out for himself and pay for it as Mr. Randolph had done. Peter Denny was not a man to harbour a resentful spirit even after such a rebuff, for he said, "in my opinion this new departure was largely, if not altogether, due to the efforts of Messrs. Randolph and Elder pushing into notice first the 'Woolf,' and then the compound engine as it is known to-day." But Peter Denny did find out for himself and did not pay for it, and early in 1868, being then the sole proprietor of William Denny and Brothers,

[1] Transactions Institute of Marine Engineers, vol. iii.

he laid down a vessel on his own account and contracted with the firm of Denny and Co. to make for her a set of compound engines. Before the vessel was half finished she was acquired by the Cunard Company and named the "Batavia." The engines were a complete success, and her owners declared that they had never had a vessel which paid better.

Ere this, however, the firm had left the old Woodyard and was established in more suitable and extensive premises on the opposite side of the Leven. About 10 acres of the present Leven Shipyard were acquired in 1864, and, after filling in the foreshore and erecting the necessary buildings, the new premises were opened in 1867. Further extensions were made in 1881 and 1882, when the full area, amounting to 43 acres, was completed.

A brief retrospect is here necessary. In the year 1846 Peter Denny was married to Helen, the eldest daughter of Mr. James Leslie, supervisor in the Inland Revenue Service. Of this estimable lady, who survives her husband, it must in justice be said that to the influence of her amiable and noble character is largely due the success of Peter Denny and the co-operation in his business of each of his talented and energetic sons. Their family, surviving, consists of John, Peter, Archibald and Leslie, besides two daughters. On William, the eldest son, coming of age, in the year 1868, he was taken into partnership with his father, and those two constituted the firm of William Denny and Brothers until 1873, when Mr. Brock joined them. In January, 1881, Mr. Denny's son John and his nephew James became partners; in January, 1883, Peter and Archibald joined the firm, and, in February, 1885, Mr. John Ward was admitted a partner. The breadth of mind and clearness of prevision which characterised Peter Denny throughout his career were distinctly manifested in the choice of his managers, and in his admitting them in due course as partners. He had a great faith in the benefit arising from an infusion of new blood into the body over which he presided, and he always acted upon the principle that the best work is given by those who have a direct interest in results. What William Denny, the younger, did for the Leven Shipyard has already been fully recorded in the Proceedings.[1] It may be said, however, that no one was more conscious of what the firm had gained by his partnership and lost by his death than was the subject of this notice. Indeed it is to be feared that the

[1] Minutes of Proceedings Inst. C.E., vol. lxxxix. p. 457.

shock occasioned by the sudden and early termination of that distinguished career left a permanent effect upon the health and vigour of Peter Denny.

Between 1868 and the present time the history of William Denny and Brothers has been one of uninterrupted progress. They are now building their five hundred and thirtieth ship, and that vast fleet includes vessels for the Cunard Company, Peninsular and Oriental Company, Royal Mail Company, Allan Line, Union Steamship Company of New Zealand, British India Steam Navigation Company, Union Steamship Company, Irrawaddy Flotilla Company, Russian Volunteer Fleet, American Line, Russian Steam Navigation and Trading Company, Compania Trasatlantica, and many other important ocean steamship lines. By no means their least significant successes have been attained with swift twin-screw and paddle-steamers for channel and river passenger service. In the preparation of the designs for these and other fast steamers the firm has been materially aided by the results obtained in their elaborately equipped experimental tank, the only device of the kind to be found in any private ship-building establishment in the United Kingdom. The experiments made upon models in wax of contemplated steamers have furnished Messrs. Denny with data of which the accuracy has been in every case established by the performances of the vessels when built. The tank and the intricate mechanical devices employed in these interesting and valuable experimental researches were the invention of the late Mr. William Froude, F.R.S.[1]

The shipyard has a frontage on the River Leven which permits of the building of eight vessels on the stocks at the same time; it has also two tidal docks, each with a set of powerful shears for lifting engines and boilers into vessels after launching. The whole of the works are served by an elaborate telephonic system, in connection with which is a signalling arrangement whereby any of the officials or foremen can be promptly brought to a telephone box in order to communicate with the head office. They have also a complete installation of electric lighting, and hydraulic and electrical power are conveyed throughout their entire extent. Ready transport is provided both by a railroad of ordinary gauge, connected with the North British system and worked by small locomotives, and by an extensive network of rails on the Decauville system which reaches every part of the

[1] Minutes of Proceedings Inst. C.E., vol. lx. p. 395.

premises. Peter Denny, aided by sons and other partners, aimed at and practically succeeded in making Dumbarton sufficient in itself for all the components of a steamship without having recourse to outside assistance. The Dennystown forge, Levenbank foundry, and Dumbarton rope-works are largely owned and controlled by Denny Brothers, and in their own shipyard they employ a competent staff of artist designers, decorators, glass-stainers, upholsterers, carvers and cabinet makers. They also supply all electric-light installations, and strive in every way to give employment to the people of Dumbarton, whose sons and daughters have the preference above all others in being afforded the means of learning the many trades which co-operate in the production of a first-class ocean passenger steamer. In 1847 the Dumbarton shipyards employed only about 400 workmen; by the year 1870 the number had increased to 2800, whereas at the present time, the shipyard and engine-works of Messrs. Denny alone, together with their forge, foundry and rope-works, give employment to no less than 3750 men, women and girls; the gentler sex being chiefly occupied as tracers in the drawing-offices, and as designers, decorators, &c., in the upholstery, stained-glass and figured panel departments.

The genius and ability of Peter Denny were potent factors in determining from the first the course taken by the firm in leading up to the foregoing results. That ability was early recognised far from Dumbarton, and in the year 1871—shortly after the lamentable loss of H.M.S. "Captain"—Peter Denny was appointed by the Government of the day as a member of the Committee on the Designs of Ships of War. Soon afterwards, in 1873, he was one of the Royal Commissioners which held sessions in that and the following year to inquire into the causes of the loss of life and property at sea. Among the knighthoods and decorations he received from foreign governments, are those of "Isabella the Catholic" of Spain, the "Illustrious Order of Jesus Christ" of Portugal, and the "Order of Leopold" from the King of the Belgians. Several years ago the University of Glasgow conferred on him the honorary degree of LL.D., and Mr. H. M. Stanley, the explorer, named a mountain in Africa "Mount Denny" after his friend the Dumbarton ship-builder. It is, however, in his native town, by the people with whom he spent his long life, and to whom therefore every side of his character was fully revealed, that Peter Denny was held in the fullest affection and esteem. In 1851 he was elected Provost of the Burgh and occupied that position for three years, during which he was chiefly instrumental

in initiating the excellent water scheme by which Dumbarton is now supplied. In 1865, before the era of Board Schools, Peter Denny erected in the town a commodious school for the gratuitous teaching of the lads employed in the ship-building, engineering and boiler-works of the firm. The condition of employment was compulsory attendance at this school, and in this generous and practical manner did he give expression to his sense of the duty devolving upon him as an employer of juvenile labour. Acting in conjunction with the late Mr. John McMillan—also a Dumbarton ship-builder—Peter Denny, in the year 1885, presented to the town the Levengrove public park, which when laid out had cost the donors upwards of £20,000. Five years later he gave further proof of his generosity by presenting to the town the large open space known as Knoxland Square, situated close to the residences of his workmen, including a handsome bandstand, from which musical performances are given from time to time in the summer months. The year 1894 was the jubilee of the firm of William Denny and Brothers, and in December the event was celebrated in the large machinery shed in the shipyard by a demonstration of the workmen and people of Dumbarton to testify to the gratitude and esteem felt for Peter Denny, the head of the firm. An illuminated address, with an album containing the signatures of his foremen and employees, was presented to him, and a silver salver, suitably inscribed, to Mrs. Denny. Shortly afterwards a movement was set on foot to obtain funds for the erection in the town of a bronze statue of Peter Denny. The money was speedily subscribed, and the work entrusted to Mr. Hamo Thornycroft, R.A. Highly as Peter Denny appreciated the sentiments towards himself which took this form of expression, it was easily seen by those who surrounded him that the erection of a statue of himself, during his lifetime at least, was not to his liking. The course of events speedily led to an issue which, amid all its sadness, served to relieve the modest, unassuming and large-hearted man of any pain on this score. In January, 1895, he took his usual winter holiday with Mrs. Denny on the Riviera, returning in April last by way of London, where he remained a few days. There he unfortunately took a chill of a malarial character. He returned to Dumbarton, and for a little time he endeavoured to resume his ordinary business duties, but he could not shake off the effects of the chill, and pulmonary complications soon set in, confining him to the house and then to his bed. A temporary improvement at one time gave grounds for hope which was, unhappily, soon dispelled. He gradually sank and passed

peacefully away on the 22nd of August, 1895, in his seventy-fourth year.

Peter Denny was an exceptional man; utterly without vanity or selfishness, he lived a life of activity and usefulness. Successful as he was in all he undertook, it is not known that he had a single enemy. Alike to the humblest of his workmen as to the most distinguished of his friends, he was ever gentle, thoughtful, generous and sincere. Throughout the whole of his business career he devoted his energies as much to the amelioration of the circumstances of those in his employment as to the building up of his own fortune. In providing wholesome and commodious dwellings for his workmen, and affording facilities for purchasing them, he made of his employees one large family, each member of which was interested in the welfare of the town and the prosperity of the business which gave them employment. Time after time did Peter Denny find by the conduct of the workmen, in periods of labour unrest, that the troubles which afflicted shipbuilders elsewhere were not brought into Dumbarton. The awards scheme for the encouragement of inventive skill on the part of his workmen; the generous contributions made by his firm to an accident fund chiefly controlled by the men themselves; the bursaries and medals given by him to encourage education in the burgh schools; and a multitude of other schemes, whereby he allowed his generosity to flow into public channels, all served to satisfy the people he employed that he was no mere exploiter of labour, but a sympathetic leader of industry and a man among the men who worked with him.

In addition to serving as President of the Institute of Marine Engineers, he was a Vice-President of the Institution of Naval Architects and a Member of the Institution of Engineers and Shipbuilders in Scotland.

He was elected a Member of this Institution on the 10th of April, 1877.

RICHARD ELIHU DICKINSON, born in Liverpool on the 16th of April, 1849, began his engineering career as an apprentice in the works of Messrs. Torres, Quiggan and Co., shipbuilders, of that city. He then entered the service of Messrs. Alexander Stephens and Co., of Glasgow, in whose drawing-office he remained for some time. In 1871 he proceeded to Bolivia, where he was engaged on the construction of a mole and piers at Tocopilla, and of a railway to that port from some copper mines in

the interior. On returning to England Mr. Dickinson commenced business on his own account at the Cleveland Engine Works, Birkenhead, where he constructed cranes and other machinery. About that time he spent several months in Portugal in erecting machinery for some mines in which he was interested.

In 1879 Mr. Dickinson closed the Cleveland Engine Works and accepted the post of manager to the Savile Street Foundry and Engineering Company, of Sheffield. At those works there were constructed, under his supervision, compound-engines and pumps, as well as some steam-tramcars, for which absence of smoke and small fuel-consumption were claimed. In 1885 Mr. Dickinson was appointed manager of the Vulcan Steel and Iron Works at Barrow-in-Furness. Two years later he removed to Jarrow-on-Tyne as manager of the Rolling Mills and Steel Works belonging to Palmer's Shipbuilding and Iron Company. There he remained until 1891, when he was appointed managing director to the Bowling Iron Company, of Bradford. During the four years Mr. Dickinson was at Jarrow he was responsible for the laying out of new steel works for Palmer's Shipbuilding and Iron Company. At the Bowling Iron Company's Works he installed a new hydraulic press; and erected at one of their largest collieries a new engine-house, air-compressors and coal-cutting machines, coal-washing plant and plant for the manufacture of steel casks welded by electricity.

Mr. Dickinson died at Bradford, after a brief illness, on the 12th May, 1895, only six weeks after his election as a Member of the Institution, which took place on the 2nd April. He was also connected with the Institution of Mechanical Engineers and with the Iron and Steel Institute.

ALEXANDER FRASER, born in London on the 15th of June, 1823, was articled—after being privately educated—to the late Mr. Henry Austin, then Resident Engineer on the Blackwall Railway, but better known, perhaps, as the brother-in-law of Charles Dickens. In the year 1845 he entered the service of the Southwark and Vauxhall Water Company as Assistant Engineer under the late Mr. Joseph Quick.[1] By the Metropolis Water Act of 1852 all the Companies drawing their supplies from the Thames were required to move the point of intake above the tidal influence at Teddington. Mr. Fraser, therefore, was engaged

[1] Minutes of Proceedings Inst. C.E., vol. cxvii. p. 383.

in assisting Mr. Quick in the design and construction of new works at Hampton for the Southwark and Vauxhall and the Grand Junction Water Companies, to the latter of which Mr. Quick was Consulting Engineer. Large additions were also made to the pumping-stations at Kew Bridge and at Battersea.

On the completion, in 1860, of the new high-service works of the Grand Junction Company at Campden Hill, which he designed and carried out subject to the approval of Mr. Quick, Mr. Fraser was appointed Assistant Engineer to that Company, of which he became Engineer on the retirement of Mr. Quick in 1876. During his long connection with the Grand Junction Company, many extensive works were completed at its various pumping-stations. Among these may be mentioned large storage-reservoirs, filter-beds and pumping machinery at Hampton; covered reservoirs for filtered water at Campden Hill and at Kilburn; a standpipe tower, reservoirs and filter-beds at Kew Bridge; and storage-reservoirs for filtered water at Mount Park Hill, Ealing. Several miles of large trunk mains and the whole of the mains in the extensive country district of the Company were laid under his superintendence. Mr. Fraser was also associated with Mr. Walter Hunter, a director of the Grand Junction Company, in the preparation of a scheme for the development of the water-supply from the Thames by the construction of large storage-reservoirs at Staines. This scheme was submitted to and approved by the Royal Commission of 1893 on the Water-Supply of the Metropolis.[1]

In 1893 Mr. Fraser suffered an attack of influenza, after which his health showed signs of failing. He struggled bravely, however, and remained at his post until within a few weeks of his death, which took place at his residence, The Mount, Ealing, on the 6th of October, 1895, the cause being attributed to dilatation of the heart. Mr. Fraser was an able engineer and displayed admirable judgment in dealing with all questions relating to waterworks, his whole energies being devoted to the interests of the Company he served so long. As a man he was genial and pleasant, and was much respected and valued by those who knew him well, but owing to his very modest and retiring disposition his services were not so fully appreciated by the general public as they deserved to be. Mr. Fraser was elected a Member on the 6th of December, 1870. In 1887 he contributed to the Institution some notes on the detection of leaks in water-mains.[2]

[1] Report of the Commission, p. 33.
[2] Minutes of Proceedings Inst. C.E., vol. xc. p. 416.

PHILIP HENRY MACADAM was the second son of the late Mr. Philip MacAdam, of Blackwater House, co. Clare, Ireland, where he was born on the 13th of February, 1831. When only eighteen years of age he migrated to the United States and entered the office of Mr. Silas Seymour, who was then New York State Engineer. After the usual routine of engineering pupilage as practised in America, during which he served on the Niagara Falls Railway survey and on the construction of the Buffalo branch of the New York and Erie line, young MacAdam was appointed an Assistant Engineer on the Attica and Allegheny Valley Railroad. At the close of 1853 he left the United States for a better appointment in Canada as an Assistant Engineer on the Port Hope and Lindsay Railway. After being engaged on survey work he was placed in charge of the construction of a division of 25 miles, on the completion of which, in 1855, he returned to Ireland. For a short period he was engaged on some land drainage schemes, but at the beginning of 1857 he again went to Canada as Resident Engineer on the Hamilton and Port Dover Railway. In the following year, however, Mr. MacAdam resigned that appointment and tendered for the construction of a portion of the line. His tender was accepted, and in 1859 he had completed the work, with the exception of laying the rails, when the Company failed. He then took an office at Hamilton and for the next three years practised on his own account in Canada. During that time he acted for Messrs. Macdonald & Co., the contractors, in the matter of a claim against the Great Western Railway Company of Canada; and made surveys and estimates for Mr. T. Brown, contractor, in connection with the Montreal Harbour improvements. In 1863, however, he again returned to Ireland, where he was once more occupied on land drainage schemes.

Mr. MacAdam's Indian career commenced in 1865, when he was appointed a second class Resident Engineer on the East Indian Railway. He was posted to the Jubbulpore branch and had charge of 30 miles of heavy construction. On the completion of that work he joined, in May, 1868, the Oudh and Rohilkund Railway as a first class Resident Engineer. The main line was then about to be commenced and Mr. MacAdam was placed in charge of a division of 60 miles. He made the surveys for this division and superintended the execution of the works thereon, retaining charge of the district after it was opened for traffic. It was in 1869, while engaged on these works, that Mr. MacAdam met with severe injuries, due to the collapse of the arched roof of an empty brick-kiln. This mishap confined him to his bed for

over two months, and necessitated the use of a crutch for eight years; and as it gradually led to a breakdown of his health, he was ultimately compelled to avail himself of a period of eighteen months' rest on furlough in Europe. In due course he returned to duty; but, although apparently restored to his former strength, the accident never failed to tell against him, and a few years ago greatly exhausted him.

In 1878–79, during the absence of his chief on twelve months' furlough, Mr. MacAdam was selected as the senior executive engineer to act in place of that officer, and later on, during his chief's second absence (this time on special duty in London connected with the erection of a bridge over the River Ganges at Benares) he was again placed in charge. During this second term his trust, for a period of eight months in 1880, was one of no light responsibility, as excessive floods did considerable damage to the line, of which about 560 miles were open, and interrupted traffic in many places. About this time the board of the Company consented to a scheme for the amelioration of the condition and prospects of the staff, Mr. MacAdam being highly spoken of and advanced as far as was then practicable. On the portion of the line actually executed by him, there were no works of special magnitude or difficulty, and he therefore had no opportunity of a display of ability; but on the railway generally, for which he was for a time twice responsible, there were many works which called for much careful thought and attention, and it may be said that wherever obstacles presented themselves, his resource was sufficient to overcome them. While firm in his control, Mr. MacAdam was ever just and considerate; and his good sense and well-merited reputation as a peace-maker led, on not a few occasions, to his being appealed to for friendly advice by juniors who fancied themselves aggrieved, and throughout his Indian career his manly and straightforward nature secured him many friends.

At the close of 1888 the Oudh and Rohilkund Railway, a "guaranteed" line, was purchased by the Secretary of State for India, and the Company's staff was disbanded. Mr. MacAdam was therefore once more free; but as by this time his health had been much impaired owing to the injury sustained in 1869, he returned to England, where he lived in retirement. The rest so gained did not, however, materially help him. His strength gradually declined; for the last eighteen months of his life he was very feeble, and finally, after a sharp short illness from ulcer of the stomach, he passed away peacefully on the 6th of July, 1895, at Southsea.

On being deprived of his appointment by the purchase of the Oudh and Rohilkund Railway, Mr. MacAdam made an appeal to the Government of India, as the successors of the Company, for a solatium for the injury sustained by him in 1869 while in the execution of his duty. He based his appeal on the fact that the accident had deprived him of that activity which would be required of him in any future appointment, and had consequently debarred him from adding to his income in the interests of his family. After much consideration, the Government of India, admitting that he was deserving of special consideration, made him a gratuity.

Mr. MacAdam was elected a Member on the 4th of February, 1873.

RALPH HART TWEDDELL, whose name is associated with the application of hydraulic pressure to the working of machine tools, was born on the 25th of May, 1843, at South Shields, where his father, Mr. Marshall Tweddell, was at that time engaged in business as a shipowner. Ralph was educated at Cheltenham College, where he was specially prepared for the Royal Military Academy at Woolwich, it being intended that he should enter the Army. The proposed military career was, however, abandoned, and in 1861 he was articled to Messrs. R. and W. Hawthorn, of Newcastle-on-Tyne. There he had ample opportunity of obtaining a practical insight into machinery of various kinds, and the encouragement he received from the firm was largely instrumental in inducing him to devote his attention to that branch of engineering with which he was subsequently so closely identified.

It was during his apprenticeship, and when only twenty years of age, that Mr. Tweddell took out a patent for a small portable hydraulic apparatus for fixing the ends of boiler-tubes in tube-plates. The pressure of water employed varied from 1 ton to 1½ ton per square inch. When the force-pump did not form part of the tool itself, the necessary connection was made by a small copper pipe, which proved sufficiently flexible and durable to allow of the free movement of the machine. The results were sufficiently encouraging to suggest the further employment of hydraulic power for machines used in boiler-construction.[1]

Owing to the introduction of high steam-pressures, the scantlings of marine boilers had to be considerably increased, and the

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[1] Minutes of Proceedings Inst. C.E., vol. lxxiii. p. 65.

mechanical riveting-machines then in use did not, in the majority of cases, make steam-tight joints. It occurred to Mr. Tweddell that the application of hydraulic pressure would overcome the difficulty, and in 1865 he designed a stationary hydraulic riveting-machine. The plant, consisting of pumps, an accumulator and a riveter, was first used by Messrs. Thompson, Boyd and Company, of Newcastle-on-Tyne, with satisfactory results. The work was done perfectly and at about one-seventh of the cost of handwork. Not only could the surplus power, when the machine was not in use, be economically applied to hydraulic presses for such purposes as "setting" angle and tee-irons, but it was found that the material was much less strained by the new process, and that the wear and tear of the moulds and dies was greatly reduced.

The difficulty, however, in many cases of getting the work to the machine caused Mr. Tweddell to turn his attention to the design of a portable riveter. When his system was proved to be practicable for portable machines of moderate weight and power, bridge-builders and others demanded that the machinery should be not only of sufficient power to do the heaviest riveting, but of sufficient gap to span the largest girders. Such conditions involved machines of considerable weight, a matter of no real difficulty, however, since the hydraulic power which actuated them furnished the most convenient means of lifting.

Messrs. Fielding and Platt, of Gloucester, undertook the manufacture of Mr. Tweddell's first portable machine in 1871, and shortly afterwards several were brought into use, among the first to employ them being Messrs. Sir W. G. Armstrong, Mitchell and Company. Two years later these machine were used for riveting *in situ* the lattice-girder bridge carrying Primrose Street over the Great Eastern Railway at Bishopsgate Street Station. The work was quickly and successfully accomplished, and since that time the plant has been used for riveting bridges in all parts of the world. In this connection the Kistna Bridge at Bezwada, the Dufferin Bridge at Benares and the Sukkur Bridge may be specially referred to. Other opportunities for applying portable machines presented themselves in rapid succession: for the riveting of locomotive boilers, gun-carriages, agricultural machinery, wrought-iron underframes for railway wagons and carriages, and considerable progress has even been made in their application to the riveting of ships.

To obtain the full advantages due to the application of hydraulic pressure to machine-tools, it is desirable that the system should be applied as far as possible throughout the works. The first

opportunity of doing this completely presented itself at the naval dockyard of Toulon. In 1874, the French government ordered iron and steel war-ships of the largest size to be built at Toulon. This necessitated the erection of new workshops, and, on the recommendation of Mr. Marc Berrier-Fontaine, Director of Naval Construction, who made an exhaustive examination of the practice obtaining in this country, Mr. Tweddell's system was adopted in its entirety. The installation was fully described by Mr. Berrier-Fontaine in a Paper read before the Institution of Mechanical Engineers at Paris in 1878.[1] This opportunity of testing the economy of the system on a large scale proved successful. A similar plant was subsequently erected at the shipyard of the Forges et Chantiers de la Loire at Penhouet, near St. Nazaire. The largest of the machines at Penhouet exerted 50 tons pressure; but one was constructed in 1883 for the naval arsenal at Brest with a pressure equal to 100 tons.

Convinced that by the use of hydraulic pressure for flanging plates much greater accuracy in the fitting and putting together of boilers would be ensured, Mr. Tweddell introduced the Piedbœuf flanging-press into this country. The economical advantages arising from the use of this class of machinery are felt not only in the process of flanging, but throughout the whole construction of a boiler.

Mr. Tweddell was always ready to make known the results of his work. To this Institution he contributed two Papers:—"On Machine-Tools, and other Labour-Saving Appliances, worked by Hydraulic-Pressure,"[2] and "Forging by Hydraulic Pressure,"[3] a process which he strongly and persistently advocated. For the former of these Papers he was awarded a Telford medal and premium. To the Institution of Mechanical Engineers he presented three Papers:—"On the Application of Water-Pressure to Shop-Tools and Mechanical Engineering Work,"[4] "On the Application of Water-Pressure to Driving Machinery and Working Shop-Tools,"[5] and "On the Application of Direct-Acting 'Pressure-Intensifying' Apparatus to Hydraulic-Presses";[6] and as a member of the Committee on the Form of Riveted Joints, he compiled a Table showing the rules of practice used by various manufacturers for riveted joints entirely in iron.[7] The Society of Arts gave him

[1] Proceedings Inst. Mechanical Engineers, 1878, p. 346.
[2] Minutes of Proceedings Inst. C.E., vol. lxxiii. p. 64.
[3] *Ibid*, vol. cxvii. p. 1.
[4] Proceedings Inst. Mechanical Engineers, 1872, p. 188.
[5] *Ibid*, 1874, p. 166. [6] *Ibid*, 1878, p. 45.
[7] *Ibid*, 1881, p. 293.

a gold medal, under the Howard Trust, "for his system of applying hydraulic-power to the working of machine-tools, and for the riveting and other machines which he has invented in connection with that system."[1] In 1890 he was awarded a Bessemer Premium by the Society of Engineers for a Paper entitled "The Application of Water-Pressure to Machine-Tools and Appliances."[2] Mr. Tweddell was a member of the French Société des Ingénieurs Civils, and he and Sir Joseph Whitworth were the only English engineers who received a *grand prix* in the machine-tool class of the Paris Exhibition of 1878. His connection with this Institution commenced in 1873, on the 2nd of December of which year he was elected an Associate; on the 25th of February, 1879, he was transferred to the class of Member.

It is difficult, perhaps, to over-estimate the importance of the change in the methods of construction of boiler, bridge and ship-building work with which Mr. Tweddell was so closely connected. Hydraulic riveting and the application of hydraulic-pressure to machine-tools may be said to have revolutionized engineering workshops. Not only is the work turned out of a better and more reliable description, but without the aid of such machinery much of that now produced could not be accomplished. Moreover, the intelligence required to direct a machine producing superior work raises the status of the labourer, while the demand created for work of this class materially increases the employment of men. In his several Papers he displayed considerable literary ability in the clear and terse language with which he stated his experience and views; and while he was careful not to speak unless he had useful information to impart, he enlivened discussion with a good-natured humour which made his remarks doubly welcome.

Mr. Tweddell died at his residence, Meopham Court, near Gravesend, on the 3rd of September, 1895, from aneurism of the heart, the result of an accident some few years ago: he was riding with his daughter, when his horse, taking fright, jumped on a rotten bank, fell and rolled over him. He was a keen sportsman and believed that he did better work for an occasional day's hunting, shooting or fishing. Mr. Tweddell was on the Commission of the Peace for Kent. He married in 1875 Hannah Mary, the third daughter of the late Mr. G. A. Grey, J.P. and D.L., of Milfield, Northumberland.

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[1] Journal of the Society of Arts, vol. xxxiii. p. 949.
[2] Trans. Society of Engineers, 1890, p. 35.

WILLIAM BINNS, born on the 4th of March, 1815, was apprenticed to the late Mr. James Ramsbottom of Todmorden. Owing however, to the stoppage in 1831 of the mechanical branch of that gentleman's business, young Binns was handed over to Messrs. Hibbert and Platt of Oldham, under whom he completed his term of apprenticeship. He was then employed by Messrs. Samuda Brothers, by Messrs. Braithwaite and Ericsson, and by Mr. James Walton of Sowerby Bridge, Halifax. In 1844 he commenced business in London as a Consulting Engineer, and was for some years engaged in the erection and inspection of machinery, mainly of steam-engines. In several instances he effected alterations in engines which resulted in increased power with diminished consumption of coal.

Mr. Binns, in addition to his private practice, held from 1847 to 1853 the post of Professor of Applied Mechanics at the well-known College for Civil Engineers at Putney. On the 16th of August, 1853, he was appointed Teacher of Mechanical Drawing to the Science and Art Department, then located at Marlborough House, and to the Royal School of Mines, Jermyn Street. In 1857 he prepared and published "An Elementary Treatise on Orthographic Projection, being a new method of Teaching the Science of Mechanical and Engineering Drawing," which was for some years recognised by the Science and Art Department as a text-book on the subject. He never failed to interest his pupils, and his practical knowledge as an engineer gave him advantages which few technical teachers at that time possessed. In October, 1863, owing to an accident which rendered him a cripple for some years, he retired from the service of the department on a small pension.

From that time Mr. Binns carried out but little engineering work. In 1867 he settled at Lake, near Sandown, in the Isle of Wight, where he lived in retirement for nearly thirty years. During the earlier part of his residence there he took great interest in local affairs, but the latter years of his life were mainly devoted to the study of astronomy. In 1876 he brought out a second edition of the treatise on Orthographic Projection. Mr. Binns died on the 31st of March, 1895, after a week's illness from influenza followed by pneumonia. He was elected an Associate on the 3rd of February, 1857, and was placed in the class of Associate Member on its creation in 1878.

MATHEW BUCHAN JAMIESON, fourth son of the Rev. Dr. Jamieson, senior minister of St. Machar Cathedral, Old Aberdeen, and younger brother of Professor Andrew Jamieson, of Glasgow, was born on the 16th of May, 1860. He was educated at "The Gymnasium," or Chanonry House School, and at Aberdeen University, and commenced his engineering career at an early age, for he was not yet fifteen, when, in November, 1874, he was apprenticed to Mr. William Boulton, the City Engineer of Aberdeen. On the termination of his pupilage, in 1879, he was appointed Chief Assistant to Mr. Boulton, which post he held for nearly five years. During that time he was engaged in the design and supervision of sewerage and waterworks, pumping machinery, bridges, and in preparing plans and valuing property in connection with an important City Improvement Scheme involving an expenditure of £133,000.

As a Student of the Institution, Mr. Jamieson prepared a Paper on "The Internal Corrosion of Cast-Iron Pipes," which was read at a Supplemental Meeting on the 11th of February, 1881, and was printed in the Minutes of Proceedings,[1] a Miller prize being awarded for it. Sir Robert Rawlinson, who presided at the meeting in question, was so favourably impressed that he took from that time a warm interest in Mr. Jamieson's career. That interest was practically displayed, when, in September, 1883, Mr. Jamieson was appointed—on the recommendation of Sir Robert Rawlinson—an Assistant Engineer in the Public Works Department of British Guiana. Three years later he became Chief Assistant Engineer, and, owing to the ill-health of his chief, was in responsible charge of the public works of the colony. These included some heavy up-country work in connection with the reclamation of land, but the most arduous part of his duties was the maintenance of the sea-board which extends for more than 300 miles. Owing to storms and to the land being below the level of the sea, this proved a difficult and even dangerous task; Mr. Jamieson had often to go at a moment's notice to a far distant part of the old Dutch works, with some 300 or 400 labourers and work night and day, until a breach was made good. He was frequently consulted as to reclamation schemes by the so-called "Sugar King of Demerara," the late Hon. William Russell, to whom he rendered great service. The work, however, was undermining his health, and mainly on this account, but partly also to be near

[1] Minutes of Proceedings Inst. C.E., vol. lxv. p. 323.

his brother William, who was engaged in mining operations and sheep-farming in New South Wales, he retired from the Public Works Department of British Guiana in the Autumn of 1888 and, proceeding to Australia, established himself in private practice at Melbourne.

William Jamieson had been a Mining Surveyor to the Government of New South Wales, and was largely instrumental in the starting of the Broken Hill Proprietary Company's Mine in 1885. His brother's influence and connection were placed at the disposal of Mathew, who, for the future, turned his attention mainly to mining engineering. He soon became engaged in the design and superintendence of crushing, concentrating, desulphurising, chlorodising, amalgamating and smelting plant. In New Zealand he designed and erected various water-races, large wrought-iron siphons for carrying water over gorges too high for bridges, Pelton wheels for driving machinery, an electric light plant, several miles of tramway rising to a height of over 2,000 feet above the works, for the purpose of bringing ore from the various mines, and a 60-stamp battery, with concentrating, desulphurising, smelting and amalgamating plant. He examined and reported upon the working and condition of several important mines, including those of the Broken Hill Proprietary Company in New South Wales. As an outcome of that connection he prepared, in conjunction with Mr. John Howell, the Company's manager, a valuable Paper entitled "Mining and Ore-Treatment at Broken Hill, N.S.W.," which was read before the Institution in May, 1893,[1] and was awarded a Telford medal and premium.

As one of a committee of two, Mr. Jamieson had charge of the carrying out of the Tarrawingee Railway (41 miles) and the Broken Hill water-supply, the two works costing considerably over £300,000. In conjunction with Mr. J. B. Mackenzie, he was engineer for two large and important schemes—one estimated to cost £280,000 and the other £72,000—for the supply of Melbourne with electricity by means of water-power taken from the Yarra Yarra river. Mr. Jamieson was also engineer to a similar undertaking in Tasmania, to provide the silver-lead mines at Zeehan with power for hoisting, pumping and ore-reduction at less than half the cost of steam-power. As one of the executive of the Mine Owners Association he had to deal with labour troubles in an acute form at Broken Hill. In the third and last strike the Association declared firmly that it would fight the Unions and, by

[1] Minutes of Proceedings Inst. C.E., vol. cxiv. p. 116.

establishing freedom of contract, be relieved from their tyranny once and for all. In this the Association succeeded after a serious strike lasting twenty-eight weeks. Previously no one but a unionist could obtain any kind of employment at Broken Hill, but now work is open to all on equal terms, and the 4000 men employed are, on an average, much better off than formerly. In place of the union, the funds of which were almost exclusively employed to maintain agitators and for fighting purposes, the owners and men have combined to form a benefit and sick fund, and for every pound subscribed by the men the owners subscribe another. The fund is managed free of cost by a committee elected from the employers and men.

Mr. Jamieson's promising career was cut short prematurely by a sudden illness on the 18th of August, 1895. He was engaged at Corryong, about 260 miles north-east of Melbourne, in examining some mines, in which he and his brother were jointly interested. He walked into the town of Corryong, some 4 or 5 miles, when, feeling unwell, he consulted a doctor, who prescribed for him and left him with the promise to return very soon. Before the doctor returned, however, Mr. Jamieson had expired.

Some idea of the energy and keen sense of duty which animated Mr. Jamieson may be gained from this brief account of his life. He showed great ability, and there can be no doubt that he would eventually have made a considerable name as a mining engineer in the Australasian Colonies. He leaves a widow (the youngest daughter of Mr. William Hall, Shipbuilder, of Aberdeen) and three young children. Mr. Jamieson was elected an Associate Member on the 1st of December, 1885, his interest in the work of the Institution showing itself in the presentation of the Papers referred to.

FREDERICK CHARLES PRESTON, second son of Mr. Frederick Walter Preston, of Burton-Latimer, Kettering, was born on the 21st of June, 1867, and was educated at Wellingborough Grammar School under Dr. H. E. Platt. He then studied for two years at the Technical College, Finsbury, where he obtained a qualifying certificate in chemistry, physics, mathematics, mechanical engineering and mechanical drawing, and laboratory and workshop experience. In 1886 Mr. Preston was apprenticed to Messrs. Black, Hawthorn & Co., of Gateshead-on-Tyne. He completed an

exceedingly satisfactory apprenticeship in the various departments in 1889, and remained with the firm as a draughtsman until October, 1892. While at Gateshead he attended evening classes at the Durham College of Science, Newcastle-on-Tyne, and in 1891 he assisted in establishing the Local Association of Students of the Institution, serving as a member of its council until he left the district.

After spending some months at sea in gaining a practical knowledge of marine engines, Mr. Preston migrated to Australia in the autumn of 1892. Owing to the general depression of trade throughout the Australasian colonies he at first suffered some rough experiences, but ultimately obtained employment at Ballarat, in Victoria, in the works of Mr. Agar Wynne. His career, however, was prematurely cut short by a severe attack of asthma, with which he was seized on returning from an exhausting journey to inspect some gold-crushing machinery. He died at Ballarat on the 8th of October, 1894, after only a few days' illness, at the early age of twenty-seven.

Mr. Preston's merits are well summed up in the following extract from a letter written after his death by Messrs. Black, Hawthorn & Co.:—"He had a thorough knowledge of locomotive, marine and general engineering, was very industrious and had abilities of a high order, and was highly esteemed by his employers for his irreproachable character, amiable disposition and exemplary conduct, both in his business and social relations." Mr. Preston was elected an Associate Member on the 5th of December, 1893.

RICHARD CARTER, son of the late Mr. Nicholas Carter, was born at Prospect House, Harrogate, on the 18th of April, 1818. In due course he was articled to a land surveyor at York, Mr. James Bulmer, under whom he obtained considerable insight into drainage operations connected with inclosures, and into the practical management of an extensive farm on scientific principles. On the expiration of his articles he undertook, with his brother Nicholas, a twelve-months' tour in the United States, after which he settled, in the year 1840, in Halifax, where he commenced to practise as an engineering surveyor. During the two following years Mr. Carter was occupied in a careful examination and survey of the district surrounding Halifax—one presenting many engineering difficulties—with a view to inter-communication by railway. In

1843 he proposed a scheme for a system of lines to be called the West Yorkshire Railway, when a rival measure was brought before Parliament, and after a fierce contest both schemes were rejected. In the following session Mr. Carter was deputed by the promoters of the two enterprises to re-survey and lay out the West Riding Union, connecting Leeds, Bradford, Halifax, Huddersfield and Dewsbury, indeed all the principal manufacturing towns of Yorkshire. This scheme was approved by Parliament and was carried out, the West Riding Union being one of the lines amalgamated on the formation of the Lancashire and Yorkshire Railway Company in 1859. It is interesting to note that while occupied on this work, Mr. Carter was closely associated with Dr. Tyndall, who at that period was engaged in surveying and levelling for a Manchester firm of railway engineers.[1]

Between 1847 and 1855, Mr. Carter devoted considerable time and attention to the question of water-supply. At Bradford he assisted in the construction of a large reservoir and other works, from which a considerable portion of the factories of that town were supplied. He acted as engineering adviser to the contractors of the Liverpool Waterworks at Rivington Pike during the construction of an important part of that extensive scheme, and he carried out works for the supply, from artesian wells, of some of the largest dyeing establishments in Yorkshire.

About the year 1856 Mr. Carter, who, with his brother, Nicholas, was interested in a large linen factory and in collieries at Barnsley, took up his residence at Cockerham Hall, on the outskirts of that town. For more than twenty years he lived there, displaying during the whole of that time the keenest interest in everything which concerned the welfare of the town, and ever ready to place his knowledge and enterprise at the service of the authorities. He was a member of the old Local Board, and when Barnsley obtained a charter of incorporation he was elected a municipal councillor, subsequently an alderman, and finally mayor, which office he served two years. During this period Mr. Carter devoted considerable time and attention to secure adequate drainage and water-supply for the borough. He was greatly interested too in geological matters, and frequently lectured and gave addresses at Barnsley, Wakefield and other places.

In 1882 Mr. Carter returned to Harrogate, where he resided for the remainder of his life. As long ago as 1860 he assisted in

[1] Minutes of Proceedings Inst. C.E., vol. cxvi. p. 340.

forming a company for the purchase of the Victoria Park estate in that town. He bought land under advantageous circumstances, the estate was rapidly developed and was laid out in fine streets and avenues on a systematic plan. An excellent site in the centre of the town was set apart for a railway station, and it was the means of bringing to Harrogate in 1862 a loop line which afforded direct communication with Leeds and other large towns in the West Riding. In the same year a branch was opened along the Nidd Valley to Pateley Bridge. Prior to 1862 the Church Fenton and Harrogate line, the Leeds and Thirsk, and the York and Knaresborough, were the railways supplying the Harrogate district, but of these the first was the only one having a station in the town, the stations for the Leeds and the Knaresborough lines being at Starbeck, about 1½ mile distant. By the loop line above referred to connection was made between the Church Fenton and the Leeds and Thirsk lines and a central station at Harrogate. These lines are now all part of the North Eastern system. Mr. Carter also largely assisted in procuring a continuous and pure supply of water for the town, his professional knowledge and intimate acquaintance with the geology of the district being readily placed at the service of the Waterworks Company, of which he was a Director. His advice was frequently sought as to the sources of the various mineral springs abounding in Harrogate, and when recently the new Montpelier Baths were about to be constructed, he was called in to examine the strata and to advise the Corporation as to how the heavy foundations might best be laid so as not to divert or injure the springs. He was a member of council of the Yorkshire Geological Society, and he delivered, as president of its meeting in Harrogate in 1884, an able address dealing with the geological formations and the mineral waters of the town and district. Mr. Carter was also Consulting Engineer for the new works for the water-supply of Ripon; the Lumley Moor reservoir was constructed under his personal supervision, and the new supply, which is by gravitation, saved the expense of the previous costly pumping operations.

Mr. Carter died at his residence, Spring Bank, Harrogate, on the 26th of September, 1895, after a short illness. A fortnight previously he had taken a chill while inspecting in heavy rain some borings for a new reservoir in the district. With characteristic energy he endeavoured to shake off the cold, and on the following day felt sufficiently well to take an active part in the opening ceremony of a local bazaar. The exertion and exposure were, however, too

great, and he was obliged to take to his bed with a feverish attack rapidly developing into congestion of the lungs, which unhappily proved fatal. Mr. Carter was a man of great ability and shrewd judgment, and he combined in a high degree considerable business capacity with extreme kindness of heart. "Mr. Richard," as he was affectionately and familiarly called, was well known, both in Barnsley and in Harrogate, to be ever ready to assist a really deserving case and to give generous support to any good object. He was one of the promoters of the charter of incorporation for his native town which was granted in 1884, and he presided at the meeting of the councillors when his brother, Mr. Nicholas Carter, was elected first mayor. In the following year a separate commission of the peace was granted to Harrogate, and Mr. Richard Carter was elected a magistrate, the duties of which office he served until his death. He was a director of the local Conservative Club, the Ruling Councillor of the Harrogate Habitation of the Primrose League and an active Freemason. Mr. Carter was elected an Associate on the 1st of May, 1855.

DAVID CHADWICK, born at Macclesfield on the 23rd of December, 1821, was the youngest of a family of nine. Very early in life he was taken to Manchester, where, after a short period of schooling, he was placed in a warehouse at the age of eleven. Anxious to do all in his power to remedy the defects arising from his early removal from school, he regularly attended evening classes. In those days warehouse hours were longer than now, and this employment of his very limited spare time showed great determination and strength of will. Throughout life he retained a strong interest in Working Men's Colleges and Mechanics' Institutes; he became a Director of the Manchester Athenæum at the age of twenty-one, and in 1858 took an active part in establishing the Salford Working Men's College.

Diligence in daily work and in nightly study soon met their reward. David Chadwick rapidly improved his position as a mercantile clerk; and in 1843 he was elected Treasurer to the Corporation of Salford, being then only twenty-two years of age. This office expanded greatly during his tenure, both in importance and emolument; and he retained it till 1860, when he determined to establish himself as a Consulting Accountant. In 1844 he

married Louisa, youngest daughter of Mr. William Bow of Broughton, and commenced residence in Salford.

In 1854 Mr. Chadwick, in conjunction with the Borough Surveyor of Salford, took out a patent for a stench-trap grid which the latter had invented. About the same time Mr. Herbert Frost invented a water-meter, and Mr. Chadwick, manifesting great interest in it and in the subject generally, wrote and submitted to the Institution a Paper "On Water-Meters,"[1] for which a Council Premium was awarded him. Six years later he formed with the inventor a small Company to work Frost's Patents of 1855 and 1857, which met with considerable success. A Paper "On the Rate of Wages in Manchester and Salford, and the Manufacturing Districts of Lancashire, 1839–59," read before the Statistical Society,[2] secured for him the friendship of the late William Newmarch, F.R.S. He was elected a Fellow and for a few years sat on the Council of that Society. About the same time he became a Member of the Society of Arts. Without pretensions to be a scientific man, he was in the habit, until recent years, of frequently attending the meetings of technical societies. In 1861 when the British Association met in Manchester he acted as Secretary of the Statistical Science Section, to which he contributed two Papers.[3]

Meanwhile his business had glided into a practical application of the Companies Act of 1862. With the aid of wealthy merchants of Manchester and the neighbouring towns, large industrial concerns were acquired and re-organised in rapid succession. The first of these was the wagon and railway-carriage works founded and owned by Mr. John Ashbury at Openshaw, near Manchester, which became The Ashbury Railway-Carriage and Iron Company, Limited, in 1863. It was followed in 1864 by still larger conversions of like character, when the important undertakings now known as John Brown & Co., Charles Cammell & Co., The Staveley Coal and Iron Company, The Sheepbridge Coal and Iron Company, The Park Gate Iron Company, Bolckow, Vaughan & Co., Palmers Shipbuilding Company, and others, took corporate form. Trusted implicitly by many of the most successful men in Manchester and the district, Mr. Chadwick became the agent through whom they made investments in some of the earliest

[1] Minutes of Proceedings Inst. C.E., vol. xiii. p. 421.
[2] Journal of the Statistical Society, vol. xxiii. p. 1.
[3] Report of the British Association, vol. xxxi. pp. 209, 256.

limited liability companies. So thorough was he in his investigations, and so careful to secure the best independent advice, that he gained and retained the confidence of the investing classes to an extent which was a remarkable tribute to his integrity and business capacity. This period of prosperity was certain to reveal the true spirit of the man. Absorbed in business to such an extent, without hobbies or private tastes, with no bent towards relaxation of any kind, except billiards—the innate autocracy of his nature, his aversion to criticism, advice, or opposition, or even to listen to caution, became the dominant characteristic of his conduct. Many men, however absorbed in work or study, seem to yearn for an external source of excitement, and Mr. Chadwick was no exception. Operating on no Stock Exchange, frequenting no race-course, putting money on no sporting event, gambling on no game of chance, he found excitement in litigiousness. In thirty-two years, between 1860 and 1892, he had no less than ten partners, from seven of whom he separated with lawsuits. In addition to these and to a heavy litigation, forced on himself and partners in 1876, by a combination of disappointed shareholders, and lasting till 1885, when judgment was unanimously given in his favour both by the Court of Appeal and by the House of Lords, he was for many years never without a pending lawsuit, either as plaintiff or defendant.

In 1865 Mr. Chadwick removed his residence from Salford to London, where he took offices first at 27 Great George Street, and subsequently in the City of London. About the time of this change, he conceived a desire to enter Parliament for Macclesfield. The first attempt was unsuccessful, but at the election of 1868 he won a seat. For twelve years he represented that borough, and although during that period he made motions for the amendment of the Companies Act of 1862, and for the more equitable adjustment of the Income Tax, he was not successful in carrying them beyond a preliminary stage. In 1880 he was unseated on an election petition. Four years previously he suddenly announced his intention to build, and equip with 10,000 volumes, a free library for Macclesfield, and this act of characteristic generosity was duly carried out. Local gratitude led to the presentation of his full-length portrait, provided by subscription, and placed, with a suitable legend, in the Macclesfield Town Hall.

Mr. Chadwick was consulted as to the erection of the Holloway College for Women at Egham, and eventually the estate was conveyed in trust to him and others. On the nomination of the founder

he also became, and remained during life, a governor of that college. He was elected President of the Manchester Statistical Society, and held the office for two years, 1865–67. He was afterwards first President of the Manchester Institute of Accountants; and on its absorption by the newly Incorporated Institute of Chartered Accountants in London in 1880, he was nominated as a member of council, a seat which was only vacated with his decease. In addition to the Papers already referred to, he was the Author of several essays on Parliamentary representation, poor rates and principles of rating, profit sharing, and joint stock companies. He visited the United States and Canada five times, and he also travelled extensively in parts of Europe, and made one visit to Egypt and Palestine.

In 1878 Mr. Chadwick married, as his second wife, Ursula, eldest daughter of Mr. Thomas Sopwith, F.R.S. After passing his seventieth year, the indications of diminished vitality were clear, accompanied as they were by impaired sight and increasing deafness. His last illness was mercifully brief. Returning home from a visit to the sea-coast, he was stricken with paralysis, which rendered him speechless. After three days' illness he died at his residence, the Poplars, Herne Hill, early on the morning of the 19th of September, 1895, in the 74th year of his age. Mr. Chadwick was elected an Associate on the 23rd of May, 1854.

THE death of SIR ROWLAND MACDONALD STEPHENSON removes from the Roll of the Institution the name of a man who, on several grounds, is entitled to an exceptional place among the biographical notices which are issued of its deceased members. He had arrived at a ripe old age, and was at the time of his death what is called "the father of the Institution," *i.e.* the person whose name had been longest on the books, having been elected on the 1st of March, 1836. He was not a "Member" in the strict sense of the word; for though he had qualified himself for the engineering profession, the use he made of his engineering knowledge was not to practise in detailed construction, but a far wider one, namely, to promote and spread the work of the engineer over the most gigantic areas of the habitable world. The Council would have had ample justification in classing him among the full Members, but he never desired to change his grade, and so

he remained an "Associate," as he was elected, to his life's end. The governing body has, however, in this notice, the opportunity of acknowledging the value of his services to the profession, which it most willingly does.

His career was in some respects analogous to that of the late Honorary Member, Ferdinand de Lesseps; for as "Le Grand Français" devoted successfully his best energies to the work of the Suez Canal, so the subject of this memoir occupied the best part of his life in the establishment of the great system of railways in India, which he lived long enough to see carried into full development. And, to follow out the analogy, as the obituary notice of M. de Lesseps[1] necessarily comprised a general history of his great undertaking, so it will be desirable here to enter into some detail as to the origin and progress of the Indian Railways, of which Sir Macdonald Stephenson (as he was usually called) may truly be esteemed the Founder.

He was a descendant of a family long settled in Cumberland and Westmoreland, its residence in those counties being traceable since 1538. He was born in London on the 9th of June, 1808, and was educated at Harrow. He entered a banking house with bright prospects, but the failure of the firm in 1828 compelled him to look for other employment, which he obtained from one of the great engineering establishments in Staffordshire, the "Gospel Oak Ironworks."

His first occupation was as their London agent, and there is no reason to suppose that, previous to that engagement, he had any experience in work allied to the profession of engineering. But this was the period of the beginnings of railways, and young Stephenson must, from his position, have heard of the doings of his great namesake. Indeed, the impulse that the railway movement was giving to engineering factories must have been felt by the firm with which he was connected, and he was thoughtful enough to try to turn this to his own advantage.

He assiduously studied the nature of the business which passed through his hands, and he supplemented this practical process by devoting his evenings to the acquirement, under experienced teachers, of mathematical and other scientific knowledge, and to the careful perusal of the best engineering works he could find, with the view of becoming an engineer. After this preparation he

[1] Minutes of Proceedings Inst. C.E., vol. cxix. p. 371.

appears to have passed a further time at the works themselves, which enabled him thoroughly to master the mechanical details and routine of engineering manufacture. There is no doubt that the years spent in this way formed a fairly sufficient qualification to enable him to become a practising engineer, had he been disposed to do so.

But he probably felt the desirability of some fixed income, for in 1838 he accepted the post of Secretary to an Association called "The Comprehensive Company for establishing regular Steam Communication with India." It is unnecessary to notice this society further than to say that it ultimately resulted in the formation of a more important body, which has now attained world-wide celebrity, namely the "Peninsular and Oriental Steam Navigation Company," of which the original Charter of Incorporation was dated the 31st of December, 1840.

There can be little doubt that the connexion of the mechanical element with the business of these undertakings formed an attraction to the young man, and it is certain that their great object, namely communication with India, first set his mind on the opening there was in the East for European engineering.

But while Mr. Stephenson was engaged in this matter he did not allow his engineering knowledge to lie unused. He happened to be fond of the drama; and in visiting the theatres, he was struck by the complicated arrangements made for the scenic preparations and changes, which appeared to demand much labour and the services of many men. It occurred to his mechanical mind that much of this complication and labour might be saved, and he devised a most comprehensive and ingenious plan for effecting the object by machinery. It was described, with a complete set of drawings, in a Paper[1] read before the Institution on the 8th of June, 1841, but it does not appear ever to have been put in practice, probably because more important objects about that time began to occupy his mind.

Sir Macdonald's connexion with the steam-boat transit to the East appears to have furnished the stimulus which set him upon the great work of his life. He had clearly come to the conclusion that improved means of communication formed the most valuable instrument which could be employed for spreading civilization, and extending the trade and prosperity of England; and having

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[1] Minutes of Proceedings Inst. C.E., vol. i. (1841) p. 153.

succeeded in bringing India into closer union with Great Britain by an improved sea passage, he saw, earlier and more clearly than any one else seems to have done, that there was a grand opening for the introduction, into that far-off but great empire, of the new system of land transit which was now effecting such wonders in the home country.

As early as 1841–2 he, having already access to the East India Company and the Board of Control by his steam communication business, broached the subject to them, and urged powerfully on them the extreme desirability of taking steps for the introduction of railways, which, he said, were more urgently needed in India than in more civilized countries; and which, he declared, would transform the most costly, slow and uncomfortable, into the cheapest, most rapid and most convenient method of transport in those widely-spread states. But though he unceasingly pressed the subject upon the authorities, he made little or no impression upon them. They hesitated to embark in what appeared to them a grave risk, and replied, in support of their objections, that white ants would destroy the timber sleepers; that constantly recurring floods would wash away the embankments; that rank vegetation would choke the line; and that the natives would certainly never travel by the railway, even if it could be made and maintained.

Mr. Stephenson's only comment upon these imaginary difficulties was, that in 1843 he gave up all work he was engaged on in England, and went out, with his wife and children, to India, where he resolutely determined to remain until he had effected his object, or at least had impressed its advantages on the authorities.

Soon after arriving in Calcutta, his talents having attracted attention, he was asked to undertake the editing and management of the leading daily paper, *The Englishman*, and gladly accepted the task, as offering considerable facilities for his plans. In January, 1844, he published an article explaining his views, illustrated by a large map of India, showing the whole system of lines which he held to be most suitable for the country.

The Governor-General, Lord Ellenborough, who regarded the scheme as visionary, gave no encouragement to it; but, on his recall, Mr. Wilberforce Bird (the acting Governor-General), and Sir Frederick Halliday (late Lieutenant Governor, but then Secretary to the Government of India), appreciating the value of

such a project, Mr. Stephenson wrote, on the 15th and 20th of July, 1844, two official letters asking the straightforward question whether, and to what extent, the co-operation and support of the Government would be granted, in the event of one or more lines of railway being undertaken by private capitalists. Sir Frederick gave an answer generally favourable, and at once published the correspondence with Mr. Stephenson in the Government Gazette "as a matter of great public importance."

Mr. Stephenson thereupon returned home, and on the 26th of November published a report, addressed to Mr. Wilberforce Bird, giving a full account of all that he had done in the matter while in India. A few extracts from this, the first important publication on the subject, will show its general nature.[1] He says—

"The development of the resources of British India had engaged my attention for a period considerably antecedent to my visiting that country, by which I was enabled to confirm by personal observation and enquiry the impressions previously entertained on the subject.

"Among my first endeavours to accomplish the object in view, in a country in which the construction and uses of a railway were scarcely known, I from time to time published, in the Native as well as in the English local journals, the reports of the various European railway companies, with statements of their expenditure and income, the traffic in goods and passengers, as well as the general effect which has been observable in every district through which a line of railway has been laid down. By these means the public mind became familiarized with the operation and advantages derived by other countries from the application of this mode of transport, which has already induced a strong and decided feeling in favour of the early introduction of similar measures in India.

"With a view to provide the necessary materials for duly estimating the relative advantages of the different lines on which, from the known extent of trade, it appeared probable that a railway would prove remunerative, I commenced a series of papers . . . which I purpose to complete hereafter.

"The time having, however, arrived for imparting to the subject a more practical and substantial form . . . I have been induced to recommend that a commencement should be made . . . without delay.

"I would also recommend that the proposed Company should be incorporated under the title of the East India Railway Company, with a view to their carrying out in succession the several lines of railway in India."

Early in 1845 a formally drawn up prospectus for the Company was put before the Court of Directors of the East India Company; and after many discussions the Court addressed, on the 7th of May, a despatch on the subject to the Governor-General, which formed the beginning of a correspondence between the Home and the Indian

[1] This Report is in the Library of the Institution.

Governments of great length and complication. As a practical measure, however, the Directors came to the conclusion that it would be advisable to send out to India a railway engineer of experience, who, in conjunction with two Indian officers, would, after due inquiry, suggest some scheme of moderate length as a first experiment. The engineer appointed for this purpose was Mr. Frederick Walter Simms,[1] who arrived in India in September 1845.

This measure was due chiefly to the influence of Mr. Stephenson with the Directors, and he accompanied Mr. Simms to India with a small staff to aid in the surveys.[2] It was his hope that Mr. Simms would disabuse the minds of the Directors of the engineering objections that had been made against the railway system. Mr. Simms sent in a memorandum in February, 1846, and this was considered and discussed by the Government of India (then under Lord Hardinge), along with a report of a "Committee of Engineers," who recommended the construction in the first instance of a line from Calcutta towards Delhi. Early in

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[1] Minutes of Proceedings Inst. C.E., vol. xxv. p. 519.

[2] It is right to say that, while Mr. Stephenson was earnestly promoting railways in Bengal, two other engineers were repeating his endeavours in the other Presidencies, namely, Mr. Heath in Madras and Mr. Chapman in Bombay. In the latter place, the East India Company, with a view to the increased prospect of public engineering works in India, had, in 1844, formed an engineering class in the Elphinstone College, and had, on the recommendation of this Institution, sent out Mr. William Pole (now its Honorary Secretary) to take the direction of it. And it is curious that exactly at the time when Mr. Simms was engaged with Mr. Stephenson in his earliest surveys, Mr. Chapman, in Bombay, had engaged Mr. Pole, with the aid of his college class, to make a survey for what afterwards was "The Great Indian Peninsula Railway." The following letter from Mr. Chapman may serve as a record of the first practical step taken towards the now great lines in Western India:—

Railway Office, Bombay.

To William Pole, Esq. 23 *Feb.* 1846.

My dear Sir,—It has become necessary to make arrangements for levelling the line of the proposed railway from this port to Tannah. Will you be so good as to inform me whether (from your class of Civil Engineering in the Elphinstone Institution or from any other quarter) you are aware of any suitable and efficient help for that purpose which can be obtained, and if any such be within your knowledge what measures will be necessary for obtaining it.—Yours very truly,

T. Chapman,

Manager Great Indian Peninsula Railway Company.

1846 Mr. Stephenson returned to England, having completed his survey, on which he also made a report,[1] describing his selection of a line.

In December, 1846, the Board of Control communicated its views to the Court of Directors, and further discussions followed at considerable length (chiefly on the terms to be granted), until August, 1849, when the Honourable East India Company formally engaged in a contract with the East Indian Railway Company for the construction of an experimental line, at a cost not exceeding one million sterling. About this time Lord Dalhousie had succeeded Lord Hardinge as Governor-General, and took great interest in the railway undertaking, making several alterations which he considered beneficial.

In the meantime Mr. Stephenson had not been idle. He went to India in 1847, returning to England in 1848, and in 1850 he went out again to supervise the actual construction of the line, for which he took out a complete and efficient executive staff. This was under the control of an eminent and experienced Chief Engineer, Mr. George Turnbull,[2] who remained at his post till the opening of the line to Benares in 1863.

Mr. Stephenson had selected as the preferable route the line from Calcutta, crossing the Hooghly river about 20 miles above that city, and continuing the direct course to Benares as the main or trunk line (to which branches might be made later on) and to Agra and Delhi. The Government of India, however, adopted the circuitous route bordering the Ganges; and some years later the correctness of Mr. Stephenson's views was acknowledged and acted upon, by the addition of the "chord line" on the alignment which he originally recommended.

The construction of the railway was now prosecuted with vigour. Mr. James Meadows Rendel[3] had been appointed the Consulting Engineer in England, and the immense provisions of ironwork and apparatus required were designed by him, and manufactured under his superintendence;—a work in which he availed himself, until his death in November, 1856, of the assistance of Mr. W. Pole.

On the 3rd February, 1855, the first portion of the line, 121 miles from Calcutta towards Delhi, was opened by Lord Dalhousie,

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[1] This Report is in the Library of the Institution.

[2] Minutes of Proceedings Inst. C.E., vol. xcvii. p. 417.

[3] *Ibid*, vol. xvi. p. 133.

the occasion being celebrated by a public breakfast at Burdwan. Lord Dalhousie had taken up the subject with an earnest determination that the country should no longer lack an element of strength and prosperity which was within grasp, and his exhaustive and valuable Minute of 20th April, 1853, addressed to the Home Authorities, terminated all difficulties and objections.[1] The further progress of this great undertaking is matter of common knowledge, and need not be described here. In 1857 occurred the memorable Indian Mutiny, with all its attendant horrors. It caused delay to the construction of the railway, and threw many of the persons engaged into difficulty and danger. One episode in this dreadful history deserves mention, namely, the heroic defence of Arrah, by Mr. Vicars Boyle (now C.S.I.), the engineer in charge of that district, which saved the lives of many gallant soldiers and civil inhabitants of the station.

Mr. Stephenson, however, escaped this danger. In 1856 his health gave way under excessive mental and physical strain, and he left India for a few months' thorough change, but he was prohibited, under medical advice, from returning, and thenceforward remained in London. On taking leave he received a valuable testimonial in India, with a strong expression of the obligation the country was under to him. H.M. Government recommended him to the Queen for a knighthood, which was graciously bestowed upon him; and the shareholders, sensible of the services which he had rendered, and of his entire disregard of all interests except their own, presented to him, out of the surplus earnings of the line above the guaranteed 5 per cent., a provision of £2500 a year for the lives of himself and his wife, by the commutation of which he was enabled to make provision for his family. The following passage from the address of the chairman, Sir Richard Strachey, to the first meeting of the company after Sir Macdonald's death, will show the estimation in which he was held:—

"I regret, gentlemen, to have to commence the observations I am about to make to you with a reference to the death of one whose name, Sir R. Macdonald Stephenson, has been associated with that of the East Indian Railway from the time of the movement which led to the formation of our Company, for which we must look back for half a century; who for many years took a leading part, first in its construction and management in India, and subsequently became a Director of the Company, holding the position of Deputy Chairman until 1888,

[1] This Minute is in the Library of the Institution.

and finally retiring from the Board, in consequence of his very advanced age, in 1892. It does not fall to the lot of many to find their anticipations of success so fully realized as that achieved by the great Undertaking, to the initiation of which Sir Macdonald Stephenson's perseverance and energy so largely contributed; and the share he had in that result should always be remembered by those who, like ourselves, are carrying on the work in which he so long participated."

The qualifications which contributed to Sir Macdonald's success in his great work have been happily commented on in a notice published shortly after his death, from which some extracts may conveniently be given here.[1] The writer, who knew him well, says, speaking of his return to Calcutta to prosecute the making of the line:—

"With feeble health, and no knowledge of any native language, he sat for years rarely stirring out of his office, driving with the energy of five men the vast concern. There were difficulties with the Government, difficulties with the native landlords, difficulties with the contractors, and, twice at least, any other man would have retired dead beat; but Macdonald Stephenson never lost heart or patience or temper with any obstacle. He wrote with a certain difficulty, in a queer way, and he was a little intolerant of fools; but he had always a plan, and always, when dealing with officials, an infinity of persuasiveness. He became the very soul of the undertaking, every engineer under him (and he had one man of genius and many able men) knew he could rely upon support, and however great the difficulties, he demanded that the work should go on, that nobody should talk of impossibilities, that non-existent labour should be imported, and that the indispensable class of minor contractors, who did not exist and could not be imported, should be created out of the ground; and so the road rolled on till it reached Delhi."

It may be added that he had self-reliance almost to a fault; but it was this which carried him through against all obstacles. And he moreover was favoured by a physical peculiarity, namely, that he was an exceedingly ready and sound sleeper, which enabled him to carry on his work in that climate when others less gifted would have succumbed.

Mr. Stephenson's success on the East Indian Railway induced him to extend his ideas, even beyond the gigantic area which had yet occupied him. He reasoned that when India had become covered with a network of railways, it would still be a long way from Great Britain, with which it had so indissoluble a political connexion. He had studied the mode of communication between the two, and found that, shortened as it had been by the passage

[1] "Spectator," 30 November, 1895.

through Egypt, the journey must always occupy weeks by sea transit. And it occurred to him that, since there was an actual stretch of *terra firma* between India and the English Channel, the construction of a railway along that land would reduce the weeks of transit to a few days. In other words, the dream of his life was not only to found an Indian Railway system, but to beat M. de Lesseps on his own ground, by opening a direct railway line from Calais to Hindostan.

This idea seems to have presented itself to him in a feasible form at an early period while he was engaged in the construction of his line. On the 1st of January, 1850, he addressed a communication to Lord Palmerston, then Minister of Foreign Affairs, with a map illustrating the further extension of Railway enterprise, by which the East and the West—India and all Europe—could be placed in direct communication by a seven days' journey. He showed the practicability, the economical cost (if honestly carried out), and the immense results to Europe and Asia, of such a project; and he informed Lord Palmerston he would endeavour to obtain the co-operation of all the foreign States; that the work should be international; that all the States it passed through should contribute more or less; that all should participate in the benefits of the necessary contracts, and that all should co-operate by local committees in the administration and working of the project. He received at once, from Lord Palmerston, letters of introduction to the Courts of Europe, and his applications were received by them all with consideration and cordiality.

He persevered for years. In March, 1855, he sent to Lord Palmerston a summary of the state of the case, and in January, 1856, Lord Dalhousie gave him, on behalf of the Government of India, an approval of his plan and a promise of assistance therein.[1]

But the time was not ripe. There were international jealousies without end, capitalists shook their heads and asked for impracticable guarantees, and nothing appears to have come of the project, except that, in 1857, the interest manifested by Mr. Stephenson in railways through Turkey, led him to accept the chairmanship of a line then being constructed from Smyrna to Aidin, afterwards called the "Imperial Ottoman Railway." But he still harped on the complete Railway Route to India. In 1859 he wrote and circulated a pamphlet entitled "Remarks upon

[1] See article "The World's Highway," in the *Calcutta Review*, March, 1856. This is in the Library of the Institution.

the practicability and advantage of Railway Communication in European and Asiatic Turkey";[1] and again in 1878 he prepared and published maps of proposed lines connecting Constantinople with Bagdad and Teheran, obviously part of his route. But there is no record of anything being really done, and although, thanks to Messrs. Siemens, there is an Overland Indo-European Telegraph, the Indo-European Railway is still a thing of the future.

Sir Macdonald's want of success on the difficult ground of the Moslem did not, however, daunt him in his endeavour to extend the blessings of railways among the less advanced peoples of the world; on the contrary, his next attempt was addressed to a great empire still farther removed from western mechanical improvement, namely China. In 1863, when the Taeping rebellion in that country had been put down by the energetic action of the late General C. G. Gordon, Sir Macdonald Stephenson, with unfailing reliance on the civilizing and healing influences of improved communication, visited the home of the Celestials, and interested the mercantile community in his proposal to introduce railways. He obtained much local knowledge from his own observation, and from personal communications with several of the Chinamen, and in 1864, on his return to England, he published a long Report in folio on the whole subject, with a large map showing a comprehensive system of railways which would, in his opinion, most suitably meet the requirements of the country, and also a few short local lines which he considered it advisable to construct at first as pioneers of the more complete system.[2] It was perhaps one of these which, having been laid down at a later time by some enterprising proprietors, was cruelly smashed to pieces by the ungrateful pig-tailed (and pig-headed) populace.

Sir Macdonald's early engineering training was of great service to him for all the work in which he was engaged, and had he followed his own inclination, he would have devoted himself to the practice of the profession; but the time and attention imperatively demanded in the administration of his large undertakings occupied him too much to permit him to combine both callings. It has already been said that he belonged to this Institution for nearly sixty years, and he served it as an "Associate of Council" during the Session 1856–57.

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[1] This pamphlet is in the Library of the Institution.
[2] This Report is in the Library of the Institution.

He was for many years the Governor of "the Copper Mines of England," an old Corporation (holding a Charter from William and Mary, dating 1692) which he extricated from difficulties and restored to prosperity.

He was, in conjunction with Dr. Jeaffreson and Mr. T. H. Hills, one of the originators of the system of ambulances now so common for the removal of patients in infectious diseases.

In speaking of Sir Macdonald Stephenson's great work, something has been said about those traits of character to which his great success may be largely ascribed: a few words may be added here as to the more general estimation in which he was held. The foundation of all was his high sense of religion; he was pronounced to be the ideal of a Christian layman; as free on the one hand from the slightest suspicion of laxity, as on the other hand from any appearance of pharisaical pretension. His ideas of probity were of the most exalted character, and the entire and high confidence placed in his incorruptibility, in positions where, with some men, temptation would have been very powerful, undoubtedly furthered in high quarters the favourable consideration of his proposals. His generosity was great, not merely in pecuniary liberality, but in allowing to every one full, or perhaps sometimes more than full, credit for motives, for ability and for good behaviour. His bright and genial manner, and quickness of humour, contributed largely to make him a favourite in society; and this was augmented by considerable musical ability.

Sir Macdonald lived for the last fifteen or twenty years in retirement, resting after his busy life and enjoying for the most part a calm and healthy old age. But he outlived almost all his fellow-workers and the long span of his life had the disadvantage of blunting in some measure the public recollection of his meritorious career. At last his health failed and his final illness so prostrated him that he kept his bed for some four months and passed away peacefully and without pain, from natural decay, in the eighty-eighth year of his age, on the 20th of November, 1895, at his residence in Tunbridge Wells. He was buried in his family grave at Kensal Green Cemetery.

Sir Macdonald was twice married; first in 1840 to Marianne, daughter of Lieut. Hederstedt, R.N., by whom he had twelve children; and secondly, to Elizabeth, daughter of Captain Bartholomew of H.M. 24th Regiment and widow of Mr. James Tindall, of Scarborough.

*** The following deaths have also been made known since the 31st of August, 1895:—

Members.

BARBER, EDMUND SCOTT; *born* 16 August, 1845; *died* 14 January, 1896. (*Cancer in the throat.*)

CHAMBERLAIN, HUMPHREY; *born* 29 May, 1846; *died* 9 January, 1896. (*Heart disease.*)

CLARK, DANIEL KINNEAR; *born* 17 July, 1822; *died* 22 January, 1896. (*The result of an accident.*)

CLERKE, WILLIAM JOHN BIRD, B.A., C.I.E.; *born* 3 February, 1838; *died* 13 February, 1896. (*Pneumonia.*)

CRABTREE, WILLIAM; *born* 10 January, 1826; *died* 21 February, 1896. (*Cardiac syncope.*)

FRASER, ALFRED; *born* 5 July, 1853; *died* 6 February, 1896. (*Rheumatic fever.*)

HOWARD, THOMAS; *born* 5 November, 1816; *died* 17 January, 1896.

LÜDERS, *Commodore* FERDINAND WILHELM WEGHORST; *born* 22 May, 1827; *died* 29 November, 1895. (*Inflammation of the lungs.*)

McCONNOCHIE, JAMES ADAIR; *born* 21 August, 1835; *died* 17 December, 1895.

MUIRHEAD, JOHN; *born* 11 March, 1846; *died* 21 November, 1895. (*Bright's disease.*)

NETHERSOLE, WILLIAM; *born* 17 December, 1829; *died* 22 December, 1895.

RICHARDSON, CHARLES; *born* 29 August, 1814; *died* 10 February, 1896. (*Paralysis.*)

ROBERTSON, GEORGE, F.R.S.E.; *born* 23 July, 1830; *died* 7 February, 1896. (*Heart disease.*)

SIMMS, DAVID; *born* 5 May, 1845; *died* 20 January, 1896.

STIRLING, PATRICK; *born* 29 June, 1820; *died* 11 November, 1895. (*Pneumonia.*)

WALLIS, GEORGE AMBROSE; *born* 25 November, 1840; *died* 20 December, 1895. (*The result of a chill.*)

YOUNG, JOHN; *born* 27 March, 1826; *died* 2 December, 1895. (*The result of a chill.*)

Associate Members.

HAYES, RICHARD FREDERIC FITZ-EDMUND; *born* 10 May, 1855; *died* 18 December, 1895. (*Diphtheria.*)

JACKSON, EDWARD WILTHEW; *born* September, 1838; *died* 2 September, 1895. (*Heart disease.*)

KNIGHTON, JOSEPH GODBER; *born* 14 June, 1851; *died* 5 August, 1895. (*Heart disease.*)

LANGDON, JAMES HENRY CORNWALL; *died* 1895. (*Pneumonia.*)

LAVERTINE, RICHARD ALOYSIUS; *born* 7 October, 1853; *died* 12 August, 1895. (*Rheumatic fever.*)

PACHECO, ALFREDO HENRIQUE; *born* 2 February, 1855; *died* 14 February, 1895.

PIRRIE, JOHN SINCLAIR; *born* 3 May, 1849; *died* 1896. (*Apoplexy.*)

POLLOCK, WILLIAM; *born* 27 July, 1864; *died* 17 October, 1895. (*Consumption.*)

QUICK, EDWARD; *born* 21 September, 1857; *died* 11 November, 1895. (*Typhoid fever.*)

STRAPP, CHARLES LEOPOL; *born* 15 October, 1867; *died* 5 November, 1895. (*Blood poisoning.*)

THOMSON, MICHAEL NICHOLSON; *born* 13 October, 1864; *died* 15 October, 1895. (*Blood poisoning.*)

WATSON, FREDERICK HOWARD; *born* 24 July, 1863; *died* 9 October, 1895.

WEEKS, THOMAS SAMUEL; *born* 7 April, 1838; *died* 14 December, 1895. (*Dilatation of the heart.*)

Associates.

ADAMS, WILLIAM ALEXANDER; *born* 26 August, 1821; *died* 31 January, 1896.

BIDDER, GEORGE PARKER, M.A., Q.C.; *born* August, 1836; *died* 1 February, 1896. (*The result of an accident.*)

BROWN, *Colonel* FRANCIS DAVID MILLETT, V.C.; *born* 7 August, 1837; *died* 21 November, 1895.

JOHNSON, JOHN THEWLIS; *born* 22 March, 1836; *died* 15 January, 1896.

LUCAS, CHARLES THOMAS; *born* 26 October, 1820; *died* 4 December, 1895.

PORTER, JOHN HENDERSON; *born* 10 June, 1824; *died* 14 October, 1895. (*Lymphangeites.*)

SUMNER, GEORGE HARLOWE; *born* 7 March, 1853; *died* 14 November, 1895. (*Fever.*)

Information as to the professional career and personal characteristics of the above is solicited in aid of the preparation of Obituary Notices.—SEC. INST. C.E., 26 February, 1896.

Sect. III.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS.

Report of the Proceedings of the French Commission on Methods of Testing Materials of Construction.

(Ministère des Travaux publics. Commission des Méthodes d'Essai des Matériaux de Construction. Première Session. Paris, 1894.[1])

This commission was appointed by the French Government in November, 1891, and its Report is contained in four large volumes.

The Commission, with Mr. Alfred Picard as President, and two vice-presidents, was divided into two sections, each having a President and three vice-presidents—A, on metals, consisting of seventy members, and B, on other materials, with thirty-eight members. The objects of the Commission were to draw up rules for testing various materials, and decide on the standards of measurement to be adopted, in order to allow of comparison of tests.

Volume 1, in addition to the constitution of the commission, contains the reports of the two sections. That of Section A is divided into four parts as follows:—

Part I. Physical tests: (*a*) Microscopic and other examination of the exterior appearance and fractures, and tests by resonance; (*b*) Determination of density and thermal and electrical conductivity; (*c*) Determination of the critical temperature, and variations of conductivity with temperature; (*d*) Study of temper.

Part II. Chemical tests: (*a*) Analysis; (*b*) Corrosion and means of protection.

Part III. Mechanical tests: (*a*) General remarks; (*b*) On samples; (*c*) The influence of heat; (*d*) The influence of duration of test; (*e*) Testing machines; (*f*) Dimensions; (*g*) Mechanical terminology, and (*h*) Technical terminology.

Part IV. Methods of Mechanical testing: (*a*) Gradually applied tests; tensile, compressive, bending, folding and doubling, torsional, shearing and punching; (*b*) Tests by blows; bending, indentation and perforation; (*c*) Study of hardness and brittleness; (*d*) Hot and cold working, hammering, &c.; (*e*) Special tests of wires, ropes, chains, rivets, pipes and tubes, and hydraulic pressure tests.

The report of Section B, which deals only with cements, limes, and kindred materials, is divided into the following Parts:—

[1] The original is in the Library of the Inst. C.E.

Part I. General considerations, choice of tests, normal tests, &c.

Part II. Tests of cement; fineness; specific weight; apparent density; analysis; homogeneity; making of sample briquettes and mortars; setting; tensile, compressive and bending tests; stability; efficiency; porosity; permeability; solubility in sea-water; and adhesiveness.

Part III. Tests of hydraulic and fat limes.

Part IV. Tests of puzzolanas, &c.

Part V. Sand for use in mortar.

Part VI. Plasters.

Part VII. General conclusions.

Volumes 2, 3 and 4 contain the special reports on which the main reports are based; volume 2 containing twenty-five reports covered by the first three parts of Section A, volume 3 eighteen reports covered by the fourth part of section A, and volume 4 the thirty-one reports under Section B. With such a large number of reports it is only possible to refer to some of the more important. The following may be selected from volume 2:—

No. 13, by — Barba. A tabulated extract from a very large number of specifications of tests for iron and steel, arranged under the following heads:—(*a*) Tensile tests; (*b*) Tests under blows; (*c*) Bending; (*d*) Folding, hot and cold; (*e*) Various cold tests; (*f*) Various hot tests.

No. 14, by L. Baclé. Testing of metals other than iron and steel with tabulated abstract of specifications for copper and its alloys.

No. 15, by — Durant. Tests of cast-iron, with extracts from various specifications and remarks, with three sheets of illustrations.

No. 16, by Ed. Sauvage. Comparison of tests of different samples of the same material, with eleven tables of tests.

No. 17, a short report by the same Author on the selection of test samples, and

No. 21, by H. Lebasteur and P. Arnould, a full paper on testing machines, with eleven sheets of illustrations.

In volume 3 the following are specially noticeable:—

No. 26, by Messrs. Barba and Duplaix, on tensile tests; an interesting report on the alterations of form, and the influence of variation of shape and conditions. Three sheets of illustrations.

No. 30, by — Durant. Bending-tests with abstracts of specifications. Five sheets of illustrations.

No. 34, by — Clerault. Testing by blows, with abstracts of specifications. Three sheets of illustrations, and

No. 39, by L. Baclé. On various cold tests, with tables compiled from various specifications, comparing these tests with the tensile tests of the material.

In volume 4 the following are perhaps the more important, of which Nos. 8, 17 and 25 are accompanied by details of a large number of tests on which the conclusions arrived at are based.

No. 8, by R. Feret. The composition of mortar for tests, and the best method of mixing.

No. 10, by P. Alexandre and R. Feret. The setting of cement briquettes.
No. 12, by R. Feret. The setting of sand-mortars.
No. 17, by P. Siméon. Compression tests.
No. 25, by P. Alexandre, P. Debray, and H. Le Chatelier. Measurement of porosity, and
No. 28, by E. Dardenne. Tests of natural puzzolanas and of trass.

In addition to the illustrations mentioned, most of the reports are accompanied by small cuts in the text.

R. B. M.

The Timbers of New South Wales. By J. V. De Coque.

(Journal and Proceedings of the Royal Society of New South Wales, 1894, p. 189.)

This Paper gives a description of the more important timbers found in the colony of New South Wales. Special prominence is accorded to the several varieties of Eucalyptus, on account of their extended use, and for the reason that, in spite of all that has been written on the subject, the knowledge of their relative merits is yet but an imperfect one.

The Author maintains that no one having an acquaintance with the timbers of the colony can afford to be dogmatic, and remarks that before committing his opinions and statements to paper, he has in each instance, as far as possible, satisfied himself of their accuracy.

The well-known difficulty of distinguishing between the many varieties of hardwood, and the utter impossibility of setting forth any fixed rule for doing so, necessitates the employment of thoroughly trained experts to pronounce on the nature and quality of the timber supplied for use on works of any importance.

The effect of natural drying or seasoning of hardwoods cut before the sap has descended is shown under various conditions by a series of instructive diagrams from which may be gathered that a round log stripped of its bark will crack and open on the side least exposed to the sun's rays; that where subject to gum veins, on these drying, the piece bounded by the veins will shell off; that all sawn timber will, during the process of drying, shrink from the side nearest the heart.

It is always advisable where possible to avoid using hardwoods of any description containing heart. The heart is invariably the weakest portion of the tree and the seat of all such defects as rot, which will afterwards extend throughout the whole piece.

Various methods for artificially seasoning the different timbers have been devised and put into practice, but so far, owing to the numerous difficulties to be overcome, no satisfactory results have been accomplished.

The process of first steaming and afterwards drying by injecting hot air into chambers specially designed seems to be the most likely

to attain the desired end, but its absolute success has yet to be demonstrated.

The following figures, showing the effect of this process on three typical hardwoods are of interest:—

	Green.		Seasoned.	
	Weight.	Size.	Weight.	Size.
	Lbs.	Inches.	Lbs.	Inches.
Blackwood	110	$12\frac{1}{8} \times 2$	52	$11\frac{1}{2} \times 1\frac{5}{16}$
Blue gum (*E. globulus*)	113	$10\frac{1}{4} \times 2\frac{1}{8}$	84	$9\frac{7}{8} \times 1\frac{7}{8}$
Stringy bark	$108\frac{1}{4}$	$10\frac{1}{4} \times 2\frac{1}{8}$	82	$9\frac{7}{8} \times 1\frac{7}{8}$

These prove that, providing no serious injury is done to the timbers, the difficulty of excessive weight, which up to the present has stood in the way of their use for many purposes, can to a great extent be overcome. It has frequently been contended that the process of steaming and hot-air drying reduces the strength of the timber to a more or less degree, but, even if this be the case, the Author holds that a little of the strength may be safely sacrificed to obtain results so satisfactory in other respects.

A detailed description of the seasoning process is given. Great stress is laid on the use of the thermometer for regulating the temperature imparted to the timber, and the Author maintains that when the correct degree of heat has been determined for each variety of timber, hardly any two of which require the same treatment, the solution of the question of successfully seasoning hardwoods by artificial means may be looked for.

The use to which different kinds of hardwood may be put in shipbuilding is given. The most suitable for this purpose are: ironbark, blue gum, spotted gum, tea-tree, swamp mahogany and colonial beech.

Ironbarks.—The strength, durability and general excellence of ironbarks are universally known and appreciated. In the trade generally this timber is divided into three classes—the white, grey and red.

White Ironbark (*Eucalyptus paniculata*) is so called owing to its pale colour when green; it however darkens during the process of drying. In comparison with the grey and red varieties, it is closer in grain and much more difficult to work.

Grey Ironbark is obtained in considerable quantities from the northern rivers of the colony, and is extensively used in the construction of large bridges. The magnificent piles, girders, &c., in lengths up to 60 feet and 70 feet, are mostly produced from this timber, which is more plentiful, and grows to greater proportions than the other two varieties. The true white ironbark is a comparatively rare timber, but the Author considers it as the most valuable of all.

Red Ironbark (*Eucalyptus sideroxylon*) is in great demand—mostly for general building purposes. The timber grows to large dimensions, but the ring shakes, which seem peculiar to one or two districts, are a serious fault, and timber showing them at the ends should be avoided.

A very common defect in all the ironbarks consists of the large round holes made by the larva of the wood-moth. Where these touch the heart of the log decay and rot rapidly set in.

Grey Gum (*Eucalyptus saligna*, var.).—This timber is not to be confounded with the blue gum (*Eucalyptus saligna*). It is a remarkably close grained, durable timber, and, except as regards strength, it makes an admirable substitute for ironbark, particularly in the construction of large beam bridges. It is very similar to red ironbark in general appearance, but the difference can be detected owing to its shortness of grain. A chip of grey gum bent between the fingers will snap instantly. The quality of this timber varies considerably, like most hardwoods, in different districts. The Author states that the best variety is found in the Hawkesbury district, particularly around Wyong and Cooranbong. Grey gum from these places has a record of thirty years in bridge members. For piles and girders it is considered one of the best timbers.

Tallow Wood (*Eucalyptus microcorys*).—For many descriptions of work this timber is superior to ironbark. It shrinks less in drying than any other hardwood; is very dense and close in grain. Tallow wood ranges in colour from a milky-white to dark yellow, hence the opportunity occurs for the substitution of other hardwoods of similar appearance. After the tree has reached maturity rot invariably sets in. In most districts the living timber is attacked, more or less, by a small insect or borer, which eats its way across the grain, and generally at right angles, all other hardwood borers, as far as the Author's knowledge extends, follow the grain of the timber. For turned and carved builders' work tallow wood is found to be superior to all other hard woods. In the erection of road bridges it ranks next to ironbark, and has been known to stand well as girders and piles without injury for twenty years. For bridge-decking it stands first.

Red Mahogany (*Eucalyptus resinifera*, Sm.).—For general building work, except in beams, this timber is the most suitable. It is deep red in colour, close in grain, and works readily. It rarely splits or cracks if cut from matured trees. It seasons quickly in sawn sizes—a quality of much importance to the builder. When seasoned white ants do not attack it where any other timber is within reach.

White Mahogany (*Eucalyptus triantha*, Link.).—This timber is similar in hue to the light-coloured tallow wood, in fact it takes an experienced eye to detect any difference between the two timbers. Unlike tallow wood it shrinks considerably in seasoning, and is much more open in grain.

Swamp Mahogany (*Eucalyptus robusta*, Sm.).—In the ground and

for ships' framing this timber is very durable, but cannot be recommended for general building-purposes and public works.

White and Grey Box (*Eucalyptus hemiphloia*, F. v. M.).—Of great value for railway sleepers and public works; bearing an excellent record for durability and tensile strength.

Red Box or Bastard Box (*Iristania confernta*, R. Br.).—The darker coloured varieties from the northern rivers are the only ones of value. They are fine in grain, and are extensively used for building-purposes.

True Red Box (*Eucalyptus polyanthema*, Schau.).—This timber is dark red in colour, is very tough, and stands well in the ground, but is too hard for buildings. It is very durable and shrinks but little.

Yellow Box (*Eucalyptus melliodora*, A. Cunn.).—Similar in character to *E. polyanthema*. It is, however, in most districts subject to concentric gum-rings.

Blue Gum and Flooded Gum (*Eucalyptus saligna*, Sm.).—Blue gum is a close-grained valuable timber for general house building. It rarely shows any disposition to split or warp after drying. Flooded gum, on the other hand, is short in grain and decays quickly. It is adapted to furniture making, but should be avoided in structures in which it is liable to be subjected to any strain.

Spotted Gum (*Eucalyptus maculata*, Hook. f.).—On account of its great elasticity is largely used for spokes and shafts for vehicles. A wide difference of opinion exists regarding the value of this timber for constructive purposes. By some it is classed as an inferior timber, whilst many people assert its good qualities. The Author is diffident about making any positive statements regarding its merits.

Blackbutt (*Eucalyptus pilularis*, Sm.).—This is a first-class timber and enjoys great popularity among architects. Except in decking, it is rarely used in the erection of bridges.

Turpentine (*Syncarpia laurifolia*, Ten.).—Regarding the merits of this timber there is a wide difference of opinion. It is extensively used for piles, particularly in rivers and harbours infested with cobra. The quality of the timber varies materially according to the nature of the ground in which it grows. Turpentine, either in a green or dry state, is generally shunned by white ants. Another strong recommendation is that it will not burn.

River Murray Red Gum (*Eucalyptus rostrata*, Schl.).—The favourable opinion entertained by many for this timber the Author considers undeserved. It shells badly, and shrinks unevenly and abnormally, frequently to the extent of 1 inch to the foot. The red gum of the sister colonies has a remarkable record for durability, and deserves the reputation it possesses.

Eurabbie or Blue Gum (*Eucalyptus globulus*, Labill.).—This timber in New South Wales is only supposed to grow in the Tumberumba district. It is largely used in tailraces, bridge-decking and girders.

Blue Gum (*Eucalyptus Maideni*, F. v. M.).—In large sizes this

timber bears an excellent record. When green it is easily worked, but with seasoning it eventually becomes as hard as bone and stands exposure well.

Mountain Gum (*Eucalyptus goniocalyx*, F. v. M.).—A clean-grained, useful timber. It stands well in water and damp places. It shrinks evenly and does not split to any great extent.

A considerable number of other hardwoods are described, but they are all, more or less, of a very inferior nature, and most of them practically worthless for works of any description.

The Paper concludes with a few paragraphs devoted to a general account of the softwoods of the colony.

J. R. B.

Tests of African Woods. By M. Rudeloff.

(Mittheilungen aus den königlichen technischen Versuchsanstalten zu Berlin, 1895, p. 133.)

Specimens of Eucalyptus rostrata and Eucalyptus globulus, and a few unnamed specimens, from a young forest near Johannesburg, Transvaal, were received for the purpose of experiment.

In sawing Eucalyptus rostrata lengthways it behaves like oak and ash; in cross-sawing it behaves like the harder limes and maple. Its average hardness may be said to lie between ash and hornbeam. In sawing Eucalyptus globulus it worked smooth like hornbeam and beech. Its mean hardness is about the same as that of hornbeam.

The specific gravity of Eucalyptus rostrata is on the average 0·637, that of Eucalyptus globulus 0·608, while a round timber of very rapid growth had a specific gravity of 0·398.

The resistances to crushing of Eucalyptus rostrata was 6,200 lbs. per square inch, of Eucalyptus globulus 6,400 lbs. per square inch, of an unnamed square timber 8,500 lbs. per square inch.

From the transverse tests made, it is deduced that the elastic limit is higher in a direction at right-angles to the annual rings than in a tangential direction. This holds for both kinds of wood. The strength in a direction at right-angles to the annual rings is greater than in a tangential direction. Deflection is not shown by these tests to vary with the direction of the load relative to the annual rings. The moduli of elasticity tangentially to the annual rings is 15,600 and 9,700 lbs. per square inch for Eucalyptus rostrata and Eucalyptus globulus respectively.

The experiments on the shearing strength are discussed at considerable length. In Eucalyptus rostrata the shearing resistance along a radial plane is only half as great as that along a tangential plane. In Eucalyptus globulus one specimen showed a similar difference, while another showed a shearing strength of the radial section 21 per cent. greater than that of the tangential section. Comparing the two kinds of wood with each other, the shearing

strength in a tangential plane of Eucalyptus rostrata is twice that of Eucalyptus globulus.

Both kinds of Eucalyptus split more easily in a radial than in a tangential plane. In Eucalyptus rostrata the difference is about 50 per cent., in Eucalyptus globulus about 56 per cent.

The Paper is accompanied by numerous tables, diagrams, and reproductions from photographs.

A. S.

The Influence of Water absorbed Hygroscopically upon the Strength of Timber. By Julius Marchet.

(Mittheilungen des k. k. technologischen Gewerbe-Museums in Wien, 1895, p. 204.)

Mention is almost invariably made in treatises on timber of the fact that the strength and elasticity of this material depend in some degree upon the amount of moisture contained in the wood; but no attempts have apparently been hitherto made to ascertain accurately the extent to which water when absorbed by wood impairs its strength. It is often impossible to protect timber employed constructively from the wet, but in such cases all that is taken into account is the effect of the water upon the durability of the wood in question. Certain investigations recently undertaken by the Author enabled him to make a series of experiments upon the strength of wood both in the dry and wet state, in order to obtain some definite information upon this subject. The wood used was three varieties of hornbeam; it was tested by compression in the form of cubes, having sides of 1·4 inch, with an area under compression of about 2 square inches. The results given were in every instance the mean of five tests. The breaking weights are set forth in two tables, which give the figures for each test. The weight needed to crush the cubes of dry wood varied from 25,794 lbs. to 19,621 lbs. The experiments were carried out with an Emery testing machine, and the figures give the point reached at which the indicator dropped back, showing that the breaking down crushing strain had been attained. The cubes treated with water were allowed to float in the same until they were completely saturated, and the amount absorbed is stated on a similar plan to that proposed by Professor Tetmajer for indicating the water in seasoned wood, namely in the form of a percentage of the weight of the dry wood. The percentage of the water in the dry wood was likewise ascertained in each case by Tetmajer's process, and this varied from 7·7 per cent. to 9·8 per cent.; this relatively small percentage shows that the wood had been well seasoned. The weight of water taken up at the end of one, three, six, eight, ten, thirteen and seventeen days is set forth in a graphic diagram, and the percentages are given in tables. Almost complete saturation was in certain cases attained at the end of the sixth day, and the Author points out that after this there was often an apparent loss of

weight, due to the soaking out of soluble constituents. Although the weight of water absorbed varied considerably in the different specimens of wood, ranging upon the seventeenth day from 45 to 79 per cent. of the weight of the wood when dry, the crushing strains were astonishingly uniform, and the loss of strength when wet ranged from 55·9 to 61 per cent. of the total strength when dry. The weights required to crush the wet wood cubes varied from 9,038 lbs. to 11,023 lbs. The Author remarks that this serious deterioration of strength, if it is found to apply also to the chief varieties of timber used in construction, upon which he is about to carry out a series of experiments, merits the careful attention of experts, especially in the case of timber structures exposed to the action of water.

G. R. R.

The Hygienic significance of Dry-Rot.[1] By Dr. Emil Gotschlich.

(Zeitschrift für Hygiene, 1895, p. 502.)

It still remains an open question whether or no the dry-rot fungus (*Merulius lacrymans*) is injurious to human beings. Recently, in the "Handbook of Hygiene," of Messrs. Pettenkofer and Ziemssen, published in 1894, it is asserted that this fungus may possibly have an injurious effect on health, and it therefore seemed to the Author advisable to undertake an inquiry into this subject. He first discusses the opinions of previous writers on this question, and cites the conclusions of Jahn, Ungefug, Poleck and others. He also gives details of the cases of illness which in their opinion were caused by the presence in dwelling-houses of the dry-rot fungus. He states, however, that it is not possible to glean positive proofs of the injurious action of this fungus from the literature of the subject, and in spite of the extent to which dry-rot is now known to prevail, there has been nothing very recently published relating to this question. He therefore thought it might be of advantage to carry out some direct experiments to test this matter thoroughly and endeavour to obtain a decisive answer to the question.

After considerable search, some houses, six in all, were found in Breslau very strongly infested with the fungus, but after careful inquiry into the health of the inmates it was not possible to trace any actual cases of illness to this source, and these investigations were moreover too limited in number to enable him to draw any general conclusions from them.

It is pointed out that the dry-rot fungus might operate injuriously upon men or animals in two ways, either by a toxic action of the mycelium, the spores, or the gaseous and other emanations; or second, by the parasitical existence of the fungus in the body itself. Both of these facts can be tested by practical experiments

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[1] Minutes of Proceedings Inst. C.E., vols. lxxx. p. 419; lxxxvi. pp. 381, 384.

upon living animals and by artificial cultivation of the fungus, and two series of investigations were accordingly undertaken. In the first set of tests a number of white mice were confined in a box which contained a large mass of dry-rot fungus. The upper part of the box was enclosed with wire gauze, and towards the end of the experiment, the fungus was in that state of decay when it gives out abundantly the powerful and disagreeable smell which Emmerich considered might be poisonous. In another experiment white mice were placed in a wide glass jar, which contained at the bottom a layer of the fungus an inch or more in depth, with which the mice were thus in immediate contact. In neither case were they in any way injuriously affected. In further tests the brown juice expressed from the fungus was subcutaneously injected into rabbits, white mice, and guinea-pigs; dogs and guinea-pigs were fed on the mycelium; rabbits and guinea-pigs were caused to inhale the spores, and the spores were injected into various animals, but in no case was it possible to trace any injury to health or to demonstrate in the body or the tissues the existence of the fungus propagated by its spores.

A second series of experiments was undertaken in order to cultivate the fungus on suitable media and to ascertain the conditions under which it will thrive, as it is obvious that only if the fungus is capable of being grown under circumstances similar to those which it would encounter in the human body is it possible for it to flourish as a parasite. The investigations, which are described by the Author, show how extremely difficult it is to cultivate this fungus artificially, as indeed Hartig has already pointed out. In fact, only one attempt succeeded, in which slightly alkaline sawdust boiled with urine was the medium employed. It was therefore necessary to make use of pieces of wood to which the fungus had already attached itself naturally, and these were kept at various temperatures from 54° to 98° F. In all cases where the temperature approached 85° to 95° the fungus dried up and shrivelled away, and, as Hartig has shown, high temperatures are speedily fatal to it. It may therefore be confidently stated that there is no possibility that this fungus could exist at blood-heat. This circumstance is borne out by the fact that it is always found to prevail in cellars and in the coolest parts of houses. The Author states in conclusion that he was unable to find that the dry-rot fungus, which is so destructive to timber, could have any injurious action upon human beings.

G. R. R.

Concrete Arch in Eden Park, Cincinnati. By F. von Emperger.

(Engineering News and American Railway Journal, October 3rd, 1895, p. 214.)

This arch was designed and erected by the Author over the Park Avenue, Cincinnati in 1894–95, and is constructed wholly of concrete. The span is 70 feet, with a rise of 10 feet, and the width is 32 feet 6 inches over all. The concrete is reinforced by eleven 9-inch rolled joists, weighing 21 lbs. per foot, spaced 3 feet apart, and supported by a cross channel at each abutment. The arch is 15 inches thick at the centre, and 48 inches thick at the abutments, which, being bedded on solid rock, are comparatively light.

The concrete in the arch was in the proportion of one part of cement to two of sand and three of stone; but this proportion was changed to one to four before the whole of the concrete had been put in, owing to the stone supplied having been broken too small. The arch was filled in in two longitudinal sections, each of which was completed in one day, gangs working from the two abutments at once. The spandrils and wingwalls were constructed of concrete formed of one part of cement to three of sand and six of stone, faced, before the concrete had completely set, with mortar about 2 inches thick, in the proportion of two parts of cement to three of sand; and this facing, except on the railing, coping and arch proper, was tinted by a 10 per cent. admixture of yellow colouring matter. The work was commenced in November 1894, and nearly completed before the winter; but the centres and false-work were not taken down till the following spring. The filling for the roadway was compressed by a 15-ton steam roller, and, though careful measurements were taken, there were no signs of settlement. The contract price for the work was £1,485, as compared with a quotation of £2,500 for a similar bridge in stone.

The Paper is illustrated by three photographic views, and a section of the arch.

R. B. M.

The Protection of Canal-Banks. By M. Möller.

(Centralblatt der Bauverwaltung, 1895, p. 240. 7 Figs.)

Prompted by the attention paid to this subject at the International Congress on Inland Navigation, Paris, 1892, the Author made experiments with the object of discovering some means of fastening to the soil the materials used for facing the slopes of canal-banks, so as to prevent their displacement by the uplifting action of frost and ice. These experiments led to the invention of the following device.

A hole, about 4 centimetres (1·6 inch) in diameter and 50 centimetres (19·7 inches) deep, is driven in the bank perpendicular

to the slope, with a pointed iron rod; and a piece of iron wire, 4 millimetres (0·16 inch) in diameter and somewhat longer than the hole, is inserted in the centre of the latter. The projecting length of wire is bent back into the hole to form a loop, having its bend slightly above the surface, and the hole is then filled with good cement-mortar.

Such an "anchor," formed in sandy and somewhat loamy soil, has, when the cement has set, a holding-power of 330 lbs.; and a layer of concrete, 1 metre square and 5 centimetres (2 inches) thick, secured by two of them, has therefore, including its weight (220 lbs.), a resistance to uplifting of 880 lbs.—as much as that of an unsecured layer of concrete four times as thick. The latter, moreover, tends to slide downward, whilst the slighter tendency of the thinner layer to do so is effectually checked by its fastenings. The lifting-force of ice is estimated not to exceed 440 lbs. per metre of slope.

A portion of the west bank of the Dortmund-Ems Canal, near Lingen, was placed at the disposal of the Author by the authorities, in order to test the efficiency of his invention. The slope (1 in 1½) was faced with a layer of concrete, formed of varying width in order to ascertain the distance above and below water-level to which it was advisable to extend the concrete. The attachment of the latter to the "anchors" was effected as follows. Wires, 2 millimetres (0·08 inch) thick, were stretched horizontally along the surface of the slope 45 centimetres (17·7 inches) apart. At every 50 centimetres (19·7 inches) of their length a hole 55 centimetres (21·7 inches deep) was driven and an anchor formed, the loop of which served to hold down the longitudinal wires. Small stone wedges were then inserted under the latter, on either side of each anchor, to raise them from the surface, and a layer of concrete 5 centimetres (2 inches) thick spread over the slope, imbedding the wires. Over other portions of the bank, wires 4 millimetres (0·16 inch) thick, placed 50 centimetres apart, were used, and the holes were driven 75 centimetres (29·5 inches) apart. The cement effectually prevents the wire from rusting, "anchors" which had stood for six months in damp soil showing no sign of rust.

Owing to the inferior materials available for making the concrete, the frost caused cracks in it, and the first trials were not altogether successful; later applications of this method at other places and with better materials, however, are stated to have stood the winter excellently.

The Author concludes by expressing, as the result of his observation, the opinion that open joints in the facing of canal-banks, immediately above and below the water-level, are to be avoided.

W. F. S.

Works of Calais Harbour.[1]

By E. THANNEUR, Ingénieur en chef des Ponts et Chaussées.

The works of Calais Harbour, previously described, were constructed under the shelter of two main cofferdams resting against the old east jetty, one of which closed the entrance to the outer harbour, and the other the outlet of the sluicing basin. The first was cut through in 1889, and the quay walls of the outer harbour have been completed by means of compressed air. The south quay has been joined to the quay of the tidal harbour by the aid of five caissons, with a total length of 443 feet. The foundation-level rises from 33 feet below the lowest low water in the outer harbour, to 20½ feet below this level in the tidal harbour. The junction between the adjoining caissons was effected by a little auxiliary caisson, lowered into the hollow formed by two recesses provided at the ends of each foundation caisson. The auxiliary caisson was gradually raised by screws as the space was filled up with concrete, which thus formed a kind of key between two caissons. The north quay has been prolonged in a straight line for about 207 feet, and then returned in the channel to form a landing quay, for a length of about 640 feet, and was finally joined, along a length of 46 feet, with the breakwater at the outlet of the sluicing basin. The foundation of the tidal quay is laid at 33 feet below the lowest low water; and it is joined to the quay of the outer harbour, which is founded 20½ feet below the lowest low water. Eleven caissons were employed for these foundations, which were carried out in the same manner as at the south quay. The total cost of these quays amounted to about £92,000.

The breakwaters of the outlet of the sluicing basin, and of the new jetty, were commenced at the same time. The latter starts from the sea embankment at a distance of 574 feet from the west jetty, and stretches obliquely out to sea; and its pierhead, raised to the same height as the pierhead of the west jetty, is 426 feet from this latter. The total length of this breakwater, from the sea embankment, is about 1,424 feet; and it was constructed by two different processes. For the first 607 feet, the system of masonry wells, sunk by the injection of water, was adopted, as previously described.[2] The foundation level is 16⅔ feet below the lowest low water along one portion, and 19¾ feet along another; and the masonry has been raised to 23 feet and 20⅓ feet respectively above the same level. The outer 817 feet were constructed by the aid of compressed air, with eleven ordinary caissons, and one caisson forming the pierhead with a superficial area of about 360 square yards. The foundations were laid 19¾ feet, 21⅓ feet, and 23 feet successively below the lowest low water as they progressed sea-

[1] Minutes of Proceedings Inst. C.E., vol. ci. p. 334.
[2] *Ibid*, p. 336.

wards. The open space, $\frac{3}{4}$ to 1 inch wide, between two caissons is simply closed by an iron pile, to prevent the sand from the shore from falling into the channel. The masonry is raised to $20\frac{1}{3}$ feet, $16\frac{2}{3}$ feet, and 13 feet respectively above the lowest low water. A timber staging rests upon the base of masonry; and its deck, situated at a general level of $31\frac{3}{4}$ feet above the lowest low water, is connected by a flight of steps with the embankment at the land end, and with the pierhead at the seaward end, whose top is $31\frac{1}{3}$ feet above the lowest low water. The masonry was nearly completed in October, 1895, and the staging was well advanced; and the work will be finished about March, 1896. At the same time, the demolition of the old jetty, and the dredging of the site it occupied are progressing so well that the new entrance can be completed in the course of 1896.

The construction of 817 feet of jetty, by means of compressed air, will have cost about £60,000; and the cost of the other portion, including the breakwaters, which were also founded on masonry wells, will amount to about £64,000. The demolition of the old works, and the dredging will involve the expenditure of £28,000.

L. V. H.

The Harbour Works of the Free Port of Copenhagen.

By H. C. V. MØLLER.

(Den tekniske Forenings Tidsskrift, Copenhagen, 1894–95, p. 97.)

The necessity for enlarging Copenhagen harbour to accommodate its growing trade, and the desire to develop that trade still further, in the face of the increasing competition of the free ports at Hamburg and Bremen, and of that to be expected from the Baltic Canal, were the chief motives for the establishment of a free port. A considerable increase of the traffic in goods intended for transshipment was anticipated, since such goods would be free of harbour and customs dues, and saving would be effected by the employment of improved appliances for handling them.

The site occupied by the new works was formerly covered by water varying in depth from 3 to 22 feet. A pier 3,100 feet long, running north and south, forms the east wall of the south basin, the largest of the three basins in the harbour. This is oblong in shape, 2,650 feet long, and 800 feet wide at its inner end. A jetty, 1,030 feet long, and 185 feet wide, projecting from this inner end, divides the south half of the basin into two portions: that on the west side of the jetty has 26 feet of water; the other, as well as the rest of the south basin and the entrance channel, 30 feet. At its north-west corner this basin is joined, nearly at right angles, by a smaller rectangular one (middle basin) having 24 feet of water, and at its north-east corner is the entrance. In the angle between the two basins, and facing the entrance, are two berths for

the steam ferry-boats running between Copenhagen and Malmo, one on each side of a central jetty. The boats thus occupy the least possible wharfage, and can conveniently enter and leave their berths. Further north is the north harbour, 1,300 feet by 620 feet, with 24 feet of water; formed by reconstruction of previously existing small private basins, and having a separate entrance. The total water-area is 60 acres, and the total land-area 90 acres. The length of wharfage is 12,350 feet, exclusive of 3,100 feet fronting the roadstead, which, however, does not form part of the free port proper. The pier, 278 feet wide at its south end, is divided longitudinally into an inner quay 206 feet, a promenade 31 feet, and an outer quay 41 feet in width. This promenade is raised 17 feet 6 inches above the quays, and beneath it is a row of cellars 820 feet long. At 1,030 feet from its south end this height is reduced to 8 feet 2 inches, the width of the outer quay being also reduced to 20 feet 6 inches. A canal for lighters, 33 feet wide, cut through the south end of the pier and crossed by five bridges, affords convenient communication between the new and old harbours, and the roadstead. The pier terminates in a leading jetty 310 feet long and 12 feet wide, ending in a round head. A breakwater protects the two entrances, and another, between two forts off the north end of the island of Amager opposite, completes the protection to vessels lying along the outside of the pier. There is also a small harbour with 12 feet of water for pleasure-boats.

An embankment of earth, faced with sheet-piling, was constructed along the site of the pier, enclosing the whole site of the south harbour, which was divided into two nearly equal areas by an inner embankment at right angles to the first. Those portions which would ultimately have to be removed were constructed as a cofferdam. Several reasons for the construction of the inner dam are given, one being that the shallower water in the south section made an earlier start upon the excavation possible, filling thus being obtained for the north portion of the outer dam, constructed in deeper water. Another was that by means of a channel formed in the top of the dam a main drain was carried across the works to the sea, this channel also serving to carry off the water pumped from the enclosures.

Four direct-acting centrifugal pumps, one 16-inch, one 13-inch, and two 12-inch, were employed. A breach, caused by a submarine spring, occurred in the outer dam when the level of the water in the south section had been lowered 12 feet, and delayed the pumping for some weeks. When pumped dry to a depth of 15 feet 6 inches, it was necessary to work the 16-inch pump on an average eleven-and-a-quarter hours out of the twenty-four to maintain the water at that level. The three other pumps were then set to work on the north section, which was pumped dry to a depth of 18 feet 6 inches; thereafter it was necessary to keep them going on an average 16½ hours a day.

In all, 1,370,250 cubic yards were excavated, the average in a

day of ten hours being 1,888 cubic yards in the north, and 1,530 in the south section. A "Lübeck" and a Belgian excavating machine were employed in the former, and another Lübeck machine in the latter. The deepening of the entrance channel to 30 feet, and other necessary dredging, was carried out concurrently with the works, the total amount dredged being about 597,000 cubic yards.

The walls of the south basin, from the coping level 7 feet 3 inches above mean tide level down to 3 feet 9 inches below it, were constructed of granite, a longitudinal culvert being formed in them for the reception of various mains. Those on the west and south sides rest upon pile foundations, protected against worms by concrete-and-iron plates (Monier system), 6 feet 9 inches by 3 feet 3 inches, bolted to the face of the bearing piles, the spaces between which were filled with rammed concrete. The greater part of this protection, however, was formed *in situ*, a network of vertical and horizontal iron rods 5 and 7 millimetres (0·2 and 0·28 inch) thick respectively, being fastened to bolts screwed into the piles, and the concrete deposited around it. The cost of this wall was £16 10*s.* per lineal foot for a depth of water of 30 feet. The east wall rests on concrete down to 25 feet 9 inches below mean water-level, below which is a layer of rubble and a thin layer of concrete; the bottom of the wall being 6 inches below the bed of the harbour. The mean thickness of this wall is about one-third the height (38 feet 8 inches), the thickness at the base being 18 feet 6 inches. Vertical joints are formed at intervals of 260 feet in the northern portion, as the ground was somewhat yielding, though not so much so as to necessitate pile-foundations. To economize material niches are formed at the back in the concrete, which is faced with roughly-squared stone, the necessary smoothness being attained by the insertion, at intervals of 18 feet 6 inches, of vertical bands of dressed granite 18 to 22 inches wide and projecting 2 inches, which are flush with the wall above. This wall cost £24 5*s.* per lineal foot.

The quantity of water in the ground behind the walls, and the occurrence of springs, made it necessary to lay a system of drainage behind the pile foundations, the water being carried off through openings in the face of the wall. When the basin was filled—which was done so gradually that the operation extended over more than five weeks—and the flow through these orifices consequently ceased, they were stopped with bags of cement-mortar deposited by divers.

The retaining-walls of the middle basin and the outer wall of the pier, as well as those on each side of the entrance, are constructed of timber protected against worms by sheet-iron. In the middle basin the wall was built in two sections, one above, and one below water, to facilitate renewal of the upper portion when necessary. A similar course was adopted for a length of 900 feet of the pier-wall, in this case because the available pile-timber, when driven to the necessary depth, was not long enough to reach to the coping.

The breakwater protecting the entrances to the north and south harbours was constructed, according to the depth of water, of one or two courses of concrete blocks resting upon layers of rip-rap and gravel, surmounted by a wall of granite rubble, faced with roughly squared stone. On the seaward side a slope of gravel was formed, above which is a layer of chalk and flints and one of granite boulders, the whole sloping 1 in 2 up to 2 feet above water-level. On the inner side is a smaller slope of chalk and flints.[1]

Considerations of speed of construction, and the lifting and transporting appliances available, led to the adoption of a hollow, box-shaped form of concrete block, externally 8 feet 2 inches long, 10 feet 3 inches wide, and 10 feet 3 inches high, constructed as follows:—The ends consisted of concrete-and-iron plates $2\frac{5}{8}$ inches thick, formed of a network of $\frac{9}{32}$ inch and $\frac{13}{32}$ inch thick horizontal, and $\frac{7}{32}$ inch vertical, round iron rods embedded in mortar consisting of 1 of Portland cement to 3 of sand. After not less than a week, they were placed in the moulds in which the rest of the block was formed, the bent projecting ends of the iron rods serving to anchor them firmly in the sides and bottom. The plates were placed 1 foot from the end of the block, so that between the adjacent end-plates of two consecutive blocks there was a space 2 feet wide in which a solid concrete block was inserted, locking the two together. In the single-course portions of the breakwater each block weighed 35 tons, the interior was filled with sand from the dredgers, up to 1 foot below the top of the block, by which the weight was increased to 47 tons, the masonry of the wall being built into the remaining space. Where two courses of blocks were required, those in the upper one had no bottom, outside the end-plates. They were placed so as to break joint with those below, and the locking-block, chamfered at the bottom, was sunk 1 foot 6 inches into the filling of the lower course. The locking-blocks in the lower course weigh 4 tons, those in the upper one $6\frac{1}{2}$ tons. Where the sides of the blocks are exposed to the action of ice they are faced with granite.

The breakwater between the two ports consists of two lengths of 1,240 and 690 feet respectively, separated by an opening 206 feet wide with 9 feet of water. It is formed of rubble deposited on each side of a row of piles, the depth of water varying from 7 to 10 feet. The leading jetty is built of concrete blocks similar to those in the breakwater.

The whole of the free port territory is enclosed by two iron fences 6 feet apart, the outer 9 feet, and the inner 8 feet high. Detailed drawings are given of the customs buildings; and the various warehouses, sheds, offices, &c., erected by the Copenhagen Free Port Company, in the construction of which concrete and

[1] A full description of this breakwater was given in a Paper read by Mr. Møller before the International Maritime Congress in London, 1893. See General Report, Sect. I, p. 35.

iron combined on the Monier system were extensively used, are fully described. They are warmed by steam from a central station. There is also a large grain warehouse and an electric central station. The Free Port Company has obtained the right to work the port for a term of eighty years, and to issue dock-warrants for stored goods, the net proceeds of the working being divided between the Company and the Harbour Board.

The whole of the machinery employed is driven, and the harbour lighted, by electricity. The installation was made by the Allgemeine Elektricitäts Gesellschaft of Berlin, under an agreement by which the entire cost was borne by that company, which charges a fixed sum per unit of power delivered. In this charge interest and amortization are included, so that the whole will, at the end of thirty years, become the property of the Harbour Company, which, however, has the right to take possession at any time on payment of a *pro ratâ* amount.

The works, which were sanctioned by law on the 31st of March, 1891, and opened on the 9th of November, 1894, were carried out by the Harbour Board, of which the Author is Chief Engineer, and the late Mr. F. W. W. Lüders, M. Inst. C.E., was the administrative head, at a cost of £550,000; a further sum of £220,000 was spent by the Company on the equipment of the port.

The original is accompanied by a plan of the harbour and ten sheets of drawings.

W. F. S.

The Harbour- and Embankment-Works at Worms from 1890 *to* 1893. By — Seibert.

(Zeitschrift des Architekten- und Ingenieur Vereins zu Hannover, p. 417, 1894.)

The condition of the harbour, &c., before the commencement of the works in question is described, and the levels are given of high and low water reduced to the Worms gauge, of which the zero is 280·50 feet, above sea-level. The rate of fall of the stream in the vicinity of Worms is 1 in 10,000.

	Feet.
The lowest recorded level on the Worms gauge is (December, 1858)	− 2·56
The mean low-water level	+ 1·15
The mean water-level	+ 4·92
The highest known flood-level (December, 1882)	+20·41

The Rhine flows past the city of Worms eastwards, at a distance of about $\frac{1}{3}$ of a mile, the intervening area comprising a low-lying tract of meadow land (+ 10·00 feet)[1] intersected by a small stream, the Giessen. The eastern portion of the town lies at a level of 15 to 16 feet, and together with the above-mentioned tract of

[1] All levels given refer to the Worms gauge unless otherwise stated.—D. G.

meadow land is liable to inundation. The Giessen, formed by the confluence of two smaller streams, the Leiningergraben and the Altbach, received a large amount of polluted water from tanneries, &c., as well as the drainage from the eastern portion of the town.

Between the drainage pumping-station, erected in 1871, at the lower Entenkropf and its outfall into the Rhine, the stream served as a float for the timber trade.

During floods the backing up of the polluted water into a large area of the town and the destruction of large quantities of fish poisoned by this water and decomposition on its subsidence, induced very unsanitary conditions; also the overflowing of the stream and inundation of the tract of meadow land, meant that during every flood the city was practically cut off from the harbour and the Rhine bank.

The general conditions consequently were such as to render the drainage of the city very difficult, and to afford no protection to the eastern or low-lying portion against floods.

A detailed description is given of the situation previous to the new works, the harbour arrangements being principally comprised in the old quay of 492 feet long, the surface of which was at a level of 14·76 feet, bounded northwards by the quay or river wall of the Worms Harbour Station-Yard. The old quay, at its southern end, was connected by a short length of river-wall with the bridge-head of the floating bridge. To the south of the bridge-head was a river-wall of rough rip-rap, with an opening in it leading to the basin in which the boats composing the floating bridge are laid up during the winter. On the old quay were two landing-stages for steamers, the one a customs pier and the other for local traffic, and a large bonded stores warehouses for grain, &c.

These arrangements were regarded as inconvenient and insufficient for the proper development of trade, and their improvements, together with the necessary works for the drainage and protection of the low-lying portion against floods, was determined upon.

The new works comprise: (1) a connection of the line of the river left bank, commencing at a point about 550 yards south of the floating bridge, and extending northwards past the winter basin and the floating bridge till joining the old quay; (2) the filling up of the Giessen throughout the portion where it flowed past the town, its junction with the Rhine being effected by the making of a short cut, at a point about ¾ of a mile above the city instead of below, as formerly; (3) a line of flood-protection dam on the southern and eastern borders of the town; (4) the construction of a timber float; (5) the construction of a harbour of commerce and refuge, capable of being enlarged hereafter; (6) the extension of railway sidings; (7) warehouses, &c.; (8) the rearrangement of pumping-machinery, &c., to meet the new conditions as regards the drainage of the town.

A very detailed description of the new works is given and of the cost of construction of every portion including the various

items of material and labour. The total outlay, exclusive of the warehouses, up to July, 1893 amounted to £142,075.

The Paper is illustrated by general plans of the harbour and vicinity before and after the execution of the works, also section of the quay wall and diagrams of the warehouses.

D. G.

Precautions to be taken in the Construction of large Reservoirs.

By — Dutoit.

(Annales des Ponts et Chaussées, June, 1895, p. 658.)

The Author commences by insisting on the necessity of preventing any percolation through the floors of reservoirs founded on strata such as marl or gypsum, which the presence of water might either soften or dissolve to an extent likely to endanger the efficiency of the structure. The Paper deals chiefly with the construction of the reservoirs at Villejuif and Montmartre, and more especially with the double floors by which percolation through cracks in the masonry is prevented from affecting the strata on which these reservoirs are built.

At Villejuif the reservoir is entirely excavated in, and founded on, gypsum and marl, with masonry walls, floor, and vaulted roof. At present only two sections, each containing over 2,750,000 gallons, have been built, but it is proposed to add two further compartments hereafter. Each section is 236 feet long, 131 feet wide, and can be filled to a depth of 16 feet 5 inches. The total thickness of the double floor is 7 feet 6½ inches, the upper and lower portions, which are each 15¾ inches thick, being separated by galleries, intersecting at right angles, 13 feet 1½ inch apart centre to centre, and oval in section, 4 feet 11 inches high and 9 feet 2 inches across, the pillars between being square and 3 feet 11 inches in their least dimension. The lower half of these galleries is rendered with 2 to 1 cement mortar, the upper half being left rough; and before the centres for the arches are fixed, the pillars are carefully dressed and rendered to a pyramidal shape at the top with cement mortar. The floor of the reservoir, as well as the external walls, is also rendered with 2 to 1 cement mortar, and thus any percolation of water through the floor is intercepted and thrown into the galleries. By means of drains and dry stone filling outside the walls any leakage through the walls is in a similar way thrown into these galleries in the floor, and thence passes to the sewers.

The reservoir is constructed throughout of millstone grit set in cement mortar, with the exception of the pillars supporting the arched roofing and the arches themselves, which are all of brick. These arches are 11 feet 7¾ inches span, 1 foot 11½ inches rise, and only 2¾ inches thick. A layer of poor concrete, 11¾ inches thick,

is spread over the haunches, the whole rendered with $\frac{3}{4}$ inch of cement mortar, and then covered with earth to a depth of $15\frac{3}{4}$ inches. The rain-water on this arched roof is drained into the reservoir.

At Montmartre, where the reservoir has a total capacity of 2,427,000 gallons, the construction of the floor is very similar to that at Villejuif; but there is an additional substratum of concrete added below the double floor, and in this pipes are laid to further prevent any possible percolation. This reservoir, which is also of masonry, is in five sections in two groups, one three, and the other two, storeys high. The lowest reservoir in each group can be filled to a depth of 16 feet 5 inches, the second to a depth of 11 feet 6 inches, while the single third-storey reservoir is 9 feet 2 inches deep. The foundation is on yellow sand, below which are beds of marl and gypsum. The galleries in the double floor are larger than those at Villejuif, being 7 feet $10\frac{1}{2}$ inches high and 6 feet $6\frac{1}{2}$ inches wide, the intermediate pillars being 6 feet $6\frac{1}{2}$ inches square at the smallest part. The whole thickness of the floor is 12 feet $5\frac{1}{2}$ inches. The concrete below the double floor, and on the surface of which the drainage pipes referred to are laid, is 2 feet $5\frac{1}{2}$ inches thick, and the masonry between the concrete and the bottoms of the galleries, and between the tops of the galleries and the reservoir floor, is $9\frac{3}{4}$ inches and $15\frac{3}{4}$ inches thick respectively. The surfaces of the galleries are plastered in a similar manner to those at Villejuif.

The Author states that the results obtained by this mode of construction are most satisfactory. The Villejuif reservoir has been in use for eleven years, and that at Montmartre for six years, during which periods only a few cracks due to expansion have shown themselves in the floors.

The Author concludes with a description of a method of staunching these expansion cracks by filling them with pure indiarubber, whereby he states that percolation is prevented without interfering with the expansion which has been the cause of the cracks.

The article is illustrated by a sheet of plans, which show very clearly the construction of the two reservoirs described.

R. B. M.

The Waterworks of Syracuse, N.Y.

By W. R. Hill, M. Am. Soc. C.E.

(Transactions of the American Society of Civil Engineers, July, 1895, p. 23.)

The population of Syracuse in 1888 was 90,000. A small portion of the city was supplied with water by the Syracuse water company, which, however, only possessed 40 miles of street mains to 172 miles of streets, and the water was unfit for domestic or manufacturing purposes. The remainder of the city was supplied

by rain-water collected from the roofs of the houses, and by spring water sold by the gallon in bottles. In that year a commission was appointed to select the best available source for furnishing the city with an abundant supply of water for trade and domestic purposes. Skaneateles Lake, 19 miles south-west of Syracuse, was fixed upon as the most desirable source; its length is 15 miles, its breath 1 mile, and its maximum depth 350 feet. Its watershed is 73 square miles, of which the lake occupies 13 square miles, and its level was 237 feet above the service reservoir of Syracuse. As it is the feeder of the Jordan level of the Erie Canal, the law authorising the city to take this water required that the storage capacity of the lake should be increased so as to store the ordinary flow of its watershed. This was effected by removing the old dam, 9 feet high, and building another, 17 feet high and 145 feet long, of rubble masonry faced with broken coursed ashlar on a platform constructed of sawn, hemlock timber, 12 inches × 10 inches, bedded in clay and concrete and covered with 3-inch planking. Six sluice doors, to supply the canal, were made through the dam. The inlet works in the lake consisted of a timber crib, 16 feet × 16 feet × 12 feet, made of 12 inches × 10 inches oak timbers, and divided into three chambers, the two outer ones being weighted with stone, and the inlet pipe being connected to the middle chamber, into which the water enters through copper wire screens of 1-inch mesh. It is situated in a depth of 40 feet of water—a trench having been dredged in the bottom of the reservoir to receive the 54-inch diameter ball and socket jointed pipes leading to the valve-chamber in the dam. From the latter starts the aqueduct proper, 19¼ miles long, the pipes being 30 inches in diameter. The thickness of the pipes up to a head of 165 feet is ⅞ inch, and it increases $\frac{1}{16}$ inch for each 40 feet additional head. There are eleven stop-valves on the aqueduct, sixteen air-cocks situated at each summit, and on the side of the stop-valves to which the pipes rise, or from which they fall, and six sluice drains in the depressions. The pipes were tested after laying, under a hydraulic pressure of at least 100 lbs. on the square inch. The aqueduct discharges into a distributing reservoir having a capacity of 100,000,000 gallons and a depth of 35 feet. The bottom and sides of the latter are covered with a 9-inch layer of concrete.

The distributing pipes in the city have been partly relaid and extended; the minimum cover is 4½ feet.

A. W. B.

The Milwaukee Waterworks Intake.

(Engineering News, New York, 19 September, 1895, p. 177.)

Since 1874 the water-supply of Milwaukee, Wisconsin, had been taken from Lake Michigan through a 36-inch pipe, extending to a crib 2,100 feet from the shore. This supply becoming insufficient,

it was decided in 1889 to construct an intake of greater capacity. The old intake pipe was laid in a trench, dredged in the bed of the lake; but as it was necessary to carry the larger pipe further out into the lake it was not deemed desirable to lay it in this manner. It was therefore decided to construct a short shore tunnel to an outer well, from which a tunnel, 7½ feet diameter, 3,200 feet long, was to be driven 140 feet below the level of the lake, ending at a shaft reaching above water-level. From this shaft two lines of submerged pipes, 60 inches diameter and 5,000 feet long, were laid, in a trench dredged for the purpose, to the intake cribs.

One thousand six hundred and forty feet of the tunnel beneath the lake was driven without difficulty, in compact clay; when a spring discharging about 1,000 gallons per minute, was encountered, and the pumps were unable to deal with it. By the help of additional pumping plant, a bulkhead of brick was built across the face, with a timber door for access, and two 10-inch pipes to pass the water through. An examination of the work, when the pumps had got the water under control, showed that the spring was coming in on the north side. The line of the tunnel was consequently diverted to the south, from a point 90 feet back from the face, but the water was again met with, after driving 75 feet, accompanied by gravel and running sand, which led to the abandonment of this diversion. It was then decided to start at a point 150 feet from the face and deflect the line to the north and upwards so as to avoid the spring. A brick bulkhead was built across the tunnel at the back of the first diversion in order to reduce the pumping, but this caused considerable leakage through the tunnel brickwork and was dispensed with. Owing to a disagreement with the contractors, the city council then determined to take over the driving of the tunnel themselves, and work was started on the heading deflecting to the north and upwards, and the tunnel gradually ascended into the rock with running sand above it. The spring was passed without trouble and the tunnel driven for 262 feet on the diverted line without much difficulty, the last 50 feet being in clay, when the strata suddenly changed and water with gravel and sand rushed into the heading, filling 34 feet of it before it was stopped by a hastily built bulkhead. A hole 60 feet deep and 10 feet diameter was found in the bed of the lake at the end of the heading. As there was no more rock to be encountered, it was decided to make use of compressed air; the hole in the bed of the lake was filled with clay and air-locks built in the tunnel. A further diversion to the north was now made to avoid the disturbed strata, and the driving restarted 12 feet back from the face, with an air-pressure of 36 lbs. on the square inch to hold the water back in the running sand; the face of the gravel when it was not being excavated was kept plastered with clay. The strata, after continuing of this nature for 600 feet, suddenly changed back again to clay, which gave no further trouble.

Difficulties were also met with in sinking the lake shaft, which consisted of cast-iron cylinders 10 feet diameter. It was first sunk,

by weighting it and pumping the sand and water from the inside, to a depth of 68 feet, but it became evident that the cribwork around the shaft was being undermined, and another method of sinking the shaft the remaining distance became necessary. A borehole made in the centre of the shaft revealed 26 feet more of sand and loam with hard clay below. An air-lock was fixed in the shaft and air pumped in, but the escape of the air round the shaft reduced the friction of the sand to such an extent that the cylinder lifted bodily 6 feet, and sand rushed in and partially filled it. It was then decided to sink a steel cylinder inside the cast-iron one by the help of air-pressure, and this was afterwards underpinned with brickwork for a depth of 30 feet, to a point 4 feet below the tunnel invert. The tunnel was driven from the shaft in hard clay and met the heading from the shore shaft. The 60-inch pipes were laid in a trench dredged in the bottom of the lake. They were floated out in lengths of 50 feet, sunk in position and joined up by a diver. The intake cribs were built on the shore, floated out and sunk in position.

A. W. B.

The Newhouse Adit at Idaho Springs.

By A. Lakes and W. H. Wiley.

(The Engineering Magazine, New York, 1895, p. 49.)

The Newhouse tunnel is now being driven at Idaho Springs, Colorado, to undermine, at an average depth of nearly 2,000 feet, a mountain of mica-schist and gneiss $3\frac{3}{4}$ miles in diameter, traversed by mineral veins carrying gold and silver, the richness of many of which has been satisfactorily proved. The tunnel is 10 feet wide by 9 feet high, with a double line of rails of 18-inch gauge, and a space between the two lines of 30 inches. Beneath this central space is a wooden drain, 2 feet wide and 14 inches deep; the top of the drain, which is even with the top of the sleepers, forming a footway. As the flow of water is now slight, this sollar is utilized for ventilation, an exhaust fan being placed at the mouth of the adit. The first 80 feet are timbered, but after that the rock was so firm that no timber was required. The rails weigh 30 lbs. per yard, and are laid upon wooden sleepers $7\frac{1}{2}$ feet long. On one side space is left for an open drain. The gradient is 5 inches in 100 feet. At intervals of about 1,000 feet, short cross-cuts are driven to serve as shelter for the miners during blasting. The alignment of the adit is maintained by means of wooden plugs with screw eyes, driven on the centre line in the roof, from which plummet lamps are suspended. The exact centre in the heading having been ascertained each day, the workmen at the face paint a vertical centre line down the heading and begin work on either side of this. With the aid of two rock-drills, working day

and night, five holes are drilled, converging inwards, on each side of the centre line, and four or five nearer the sides of the tunnel. The central holes are 9 to 10 feet deep, and the side ones 6 to 8. Dynamite is the explosive used, 100 to 175 lbs. being required for each round. Twenty-six men are employed, divided into two shifts of drill-men, and three for handling the rock. The Paper is illustrated by twelve photographic views showing the progress of the work.

B. H. B.

On a new kind of Well in the Granitic Rocks of Sweden.

By — Nordenskiold.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxx., 1895, p. 857.)

To remedy the inconvenience arising from the lack of good drinking water in the pilot stations and lighthouses on the coast of Sweden, the Author proposed to bore wells in the granite in the hope of finding water-bearing fissures at no great depth. He was led to make this suggestion by the consideration that the variations of temperature at the surface must tend to the formation of horizontal fissures in the rock.

An experiment made at the pilot station of Arkô in 1894 was completely successful. A diamond borer, 65 millimetres (2½ inches) in diameter was used, and a plentiful supply of water was met with at a depth of 33 metres, *i.e.*, 30 metres below sea-level. Since then the attempt has been made in six other places, water of good quality being reached in all cases at a depth of from 33 metres to 35 metres, and rising generally to within 2 metres or 3 metres of the surface.

The uniformity in depth of the water-bearing fissures is adduced by the Author in confirmation of his theory as to their probable origin. The strata traversed were gneiss, granite, diorite, &c. In selecting a place for the well, surface fissures should be avoided.

G. J. B.

The Spontaneous Separation of Iron from Subsoil-Water, and a Method for treating Well-Water. By Dr. A. Lübbert.

(Zeitschrift für Hygiene, 1895, p. 397.)

A freshly drawn sample of water, rich in protoxide of iron and which may also contain oxygen, when allowed to stand undisturbed in an open vessel, parts with the iron, and in so doing is found to pass through two distinct phases. The higher the temperature the more rapid is the advent of a yellowish-brown tinge and a slight opalescence, heralding the separation of iron.

But while in the bottom of the vessel the deeper layers are attaining to the maximum degree of opalescence under the first phase, the portion nearest the surface, in undergoing the second phase, assumes an intense red-brown colour; flakes of the hydrated peroxide of iron sink; the liquid becomes more and more turbid, and it no longer becomes possible to distinguish between the second phase which so evidently begins in the zone nearest the surface and the first action which pervades the entire volume. When the sediment has all been deposited the water again becomes clear, except that, perhaps, in some cases, a thin iridescent film may partially cover the surface. The Author points out that it is obvious from these facts that the separation of the iron may take place in the deeper strata of the water before the oxygen in the atmosphere has had time to penetrate through the entire volume of the liquid, and, in the case of waters which already contain, along with the iron oxides, a supply of oxygen in solution, it is frequently found that there is quite sufficient oxygen present in the water to cause the iron to become fully oxidised. The question is asked why this action does not take place in the subsoil. The answer would seem to be that there must be some agency present which hinders it, and the Author states that, if an explanation could be found, it might render it possible to provide a means of removing the iron, without the need of having recourse to subsequent mechanical treatment, such as has been adopted in the case of waters of this character. As the result of numerous experiments, it is proved that the precipitation of the iron can only take place when the carbonic acid gas, at the same time present, has fallen below a certain fixed ratio as regards the volume of the water.

Some carbonic acid gas is needed for this action, and the hydrated peroxide of iron combines with this carbonic acid, and it can be proved that the separation of the iron from the water is actually dependent upon the degree of tension of the dissolved carbonic acid gas. It would thus appear that it might be possible to introduce into wells substances which might influence the amount of carbonic acid present in the well-water, and thereby lead to the removal of the iron oxide in the well itself, and various substances, charcoal, chalk, &c., are proposed as suitable for the purpose. Mr. Steckel of Breslau has patented a plan for removing the iron which involves the application of caustic lime. He deals with this substance in the following way:—Two thin walls of porous tiles are constructed near the exterior lining of the well, so as to form two concentric circles about 4 inches apart, and the space between them is filled with air-dried slaked lime. This interior lining is brought up to such a height as to be above the upper level of the water in the subsoil. The bottom of the well must also be lined with a layer of lime about 4 inches in depth, covered over with sand. All the water entering the well must thus encounter the lime. The suction pipe of the pump can then be placed in position and the well may be covered in. The well

should be pumped out once or twice daily for a short time before being put into use. From actual observation of a well-water, formerly rich in iron, which underwent this treatment, it is stated that during seventeen years it has remained free from iron, although in daily use for a large establishment and a garden. Analyses of the water are given, which show that though it became slightly harder under this process, the amount of iron was infinitesimal. The lime in solution was about doubled. The Author asserts that the treatment can be employed with permanent success for all waters containing iron in solution.

G. R. R.

The Steam-disinfection and Sterilization of Wells and Tube-Wells. By Dr. MAX NEISSER.

(Zeitschrift für Hygiene, 1895, p. 301.)

The Abyssinian tube-well has ousted the former shaft-well, because it can be so much better protected against surface impurities, and even should it become polluted it can, as Professor Fränkel[1] has pointed out, be readily disinfected by chemical means, whereas the possibility of the disinfection of the old-fashioned well in a similar way is, to say the least, extremely doubtful. It has been suggested by Professor Koch that it would be expedient to convert all wells into tube-wells, or at any rate to close them at a depth of from 6 feet to 7 feet from the surface with an iron plate, thickly covered with earth. The number of wells of the old defective type now in use is so vast, and the danger of pollution is so constant, that in many districts they become the hotbeds of cholera and typhoid fever, and, failing a certain and safe means of disinfection, it often becomes necessary during epidemics to order the closing of these wells—a measure attended with much difficulty, and constantly rendered inoperative by ignorance and obstinacy. Moreover, in consequence of the lack of ready means of ascertaining the entire disappearance of the germs of infection, it is frequently considered expedient to prevent the use of such wells for considerable periods after the epidemic has passed away. Hitherto the methods advocated for the disinfection of wells have been by the employment of chemicals, and the use of lime has been proposed for this purpose. Dr. Fränkel experimented with sulpho-carbolic acid, but obtained unsatisfactory results in common wells. His method of procedure was to infect the well-water with cultures of readily recognised germs, then to employ disinfectants and to test the water bacteriologically at various intervals. The Author cites also the experiments of Bratanowicz, who was likewise unable by chemical means—peroxide of hydrogen, lime, pyoktanin, &c.—to sterilise well-water infected with micro-organisms. The suc-

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[1] Minutes of Proceedings Inst. C.E., vol. xcvii. p. 456.

cessful disinfection of any well can be predicted when the germs, previously introduced, entirely disappear; such germs must, however, be capable of existence and development in the water, and they should resemble in their life-habits the pathogenic germs formerly present; they must also be such as can readily be identified when appearing in small numbers upon plate-cultures.

The experiments conducted by the Author complied with these conditions. He made use of a well about 20 feet deep, supplying a pump situated in the courtyard of the Hygienic Institute of the Breslau University. The well, which was steined round, was 3 feet 7 inches in diameter, and the average volume of water contained in it was about 400 gallons. The germs of the *bacillus prodigiosus* were employed, and later a bacillus resembling that of typhoid fever was also used. An account is given of the behaviour of the spring supplying the well when the water was pumped out in large quantities, and an investigation was made with proper precautions of the germ contents of the inflowing water, which varied very widely from 150 germs to 65,000 germs per cubic centimetre. After it had been duly ascertained that the added germs could not be wholly removed by pumping and that they had attained a settlement in the well, disinfection took place by means of sulphuric acid, as recommended by Iwanoff and Stutzer.[1] In spite of all efforts it was found to be impossible to secure disinfection by these means. Details are given in Tables of four sets of experiments, which show that though, after the introduction of the acid, there was a temporary diminution in the numbers of the germs, the effects soon passed off and the numbers of bacilli again increased. Notwithstanding the failure with the acid, lime was next employed, as suggested by Liborius and Pfuhl,[2] and 66 lbs. of freshly slaked lime were introduced into the well and the sides were carefully washed down with the milk of lime. This treatment gave better results than the use of acid, but it proved ineffectual, and showed that chemical disinfection was not to be relied upon. There still remained the trial of steam, advocated by Professor Flügge. As no steam-boiler was available, a portable locomotive with a boiler-pressure of four atmospheres was employed, and the steam was passed down direct into the well-water by means of an india-rubber pipe. The water had, as on previous occasions, been infected twenty-four hours beforehand with the germs of *bacillus prodigiosus*. It took two hours and twenty-five minutes to raise the temperature of the well-water from 50° F. to 205° F. by blowing in steam, and during this time the hot water was pumped up so as to disinfect the suction pipe and the pump-gear. At the commencement of the disinfection the water in the well contained 100,000 germs per cubic centimetre, among which were many specimens of *prodigiosus*. When the steaming was stopped the water was sterile, and for two days the water

[1] Minutes of Proceedings Inst. C.E., vol. cxiii. p. 415.
[2] *Ibid*, vol. cxii. p. 424.

pumped out was comparatively free from germs; for a period of eight days it contained no *prodigiosus* germs. This steaming process was thus the only system of disinfection which destroyed the specially introduced bacilli. A second series of experiments with steam, under conditions described by the Author, likewise proved fatal to the test-bacilli, and it was evident that steam had succeeded where the powerful chemical substances, sulphuric acid and lime, had failed. This plan of steaming wells could readily be employed in country districts where portable locomotives are available, and the total cost of disinfection would not exceed 30*s.* per well. Attention is called to the ready adaptability of this process in the case of tube-wells, and to the advantages which could be gained in testing the subsoil-water of any district by means of such tube-wells, which could be sterilised with steam in a few hours. The results of some actual tests of tube-wells are given. The analyses are set forth in tabular form in the Appendix.

G. R. R.

The Preparation of Germ-free Drinking-Water by means of Calcium Chloride. By Dr. Bassenge.

(Zeitschrift für Hygiene, 1895, p. 227.)

The disinfecting properties of calcium chloride have been frequently investigated, and a brief account is given of the opinions expressed by those who have previously dealt with this subject. Traube has pointed out its probable value for the production of a germ-free drinking water, and has proved its utility by practical experiments. His tests, however, were not conducted with pathogenic germs, though he concluded from his experiments that this substance, used in the proportions which he advocates would be fatal to all kinds of bacilli. Messrs. Sickenberger and Kaufmann have reported on the use of hypochlorite of sodium, which depends for its action, as does the calcium chloride, on its available chlorine, and the former of these Authors points out the advantage in the use of the soda salt, in that it does not add to the hardness of the water. As, however, both these substances, as commercially prepared, vary in the amount of chlorine they contain, it is necessary to calculate the contents in active chlorine, and it was found that in certain proportions (5 milligrams per litre), chlorine was fatal in five minutes to all germs. Even the slightest traces of chlorine and hypochlorous acid when present in water give it a strong taste and render it unfitted for drinking purposes. Traube has shown that this chlorine might be removed by the addition of small quantities of sodium or calcium sulphites which become oxidised into the sulphates. The Author, on the recommendation of Dr. Proskauer, employed in lieu of these the common commercial bisulphite of lime, which, owing to the free sulphurous acid it

contains, must have an immediate reducing reaction upon the calcium hypochlorite, leading to the formation of calcium sulphate and calcium chloride. The Author recapitulates the experiments of Traube, and states the quantities of salts he employed. He then indicates the experiments undertaken in his own case with the substances named, and gives the results of a long series of bacteriological tests with the bacilli of typhoid fever and other germs, which were destroyed after a brief exposure to the chlorine. He sums up the facts established by him as follows:—

(1) In order to render water which is highly polluted with pathogenic bacteria germ-free, it suffices to add to it for ten minutes 0·0978 gram of active chlorine per litre, which is about equivalent to 0·15 gram (three parts per 20,000) of commercial calcium chloride. If the time under treatment is extended, say to two hours, 0·0108 gram of active chlorine is sufficient.

(2) The superfluous chlorine not needed for the disinfection can be reduced by means of calcium bisulphite, which leads to the formation of a scanty precipitate of calcium sulphate. Water treated in this way is quite innocuous, acquires no unpleasant taste, but increases in hardness. It can be partaken of throughout lengthened periods without injury to health, since, by the foregoing chemical treatment, no substances are added beyond such as are usually present in potable waters.

(3) It does not require any chemical test to determine whether all the chlorine has been removed, since this fact can readily be ascertained by taste and smell.

(4) This process of safely preparing by chemical means a germ-free drinking water can be easily carried out, and possesses from certain aspects a most important practical significance.

G. R. R.

The Cholera Outbreak in the district of Oppeln in 1892–94.

By Dr. E. Roth.

(Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege, 1895, p. 569.)

During the interval which has elapsed since the former epidemic in 1872–74, this district has remained free from cholera. In August, 1892, an imported case from Hamburg entailed the attack of a second person, and during 1893 certain cases, brought over doubtless from Russia, where cholera was very prevalent, came under notice. An account is given of several suspicious attacks from October to December, 1893, but the disease in Upper Silesia must at that time have been of a mild character. A case was discovered quite accidentally at Janow, where, in consequence of suspected poisoning, a post mortem investigation was held, and after a second person had fallen ill, a bacteriological analysis proved the attack to have been one of cholera. The family where this

death and illness occurred consisted of eight persons, and the healthy and the infected members all slept together in one bed. During 1894 the disease was raging in Galicia, and already in October of that year there had been 11,000 persons attacked, of whom 6,093 died. Oppeln was thus threatened on both sides. Isolated cases are quoted, resembling that at Janow, where it was impossible to determine the source of the disease, but in one instance, namely, at Myslowitz, in May, 1894, the origin of the infection was placed out of doubt. An account is given of the various cholera cases from May onwards, but it was not until August that the outbreak became serious. On the 16th of August, 1894, a smuggler who frequently passed into Russian-Poland was attacked with cholera at Rosdzin, and from that time onwards the disease may be said to have become epidemic. The Author describes the manner in which the infection was probably passed from place to place, and the precautions taken to prevent the spread of the malady, the officials having wide powers to enforce the most stringent measures. Even healthy suspected persons were placed apart, kept under close observation and fed at the expense of the authorities; their fæces being daily examined bacteriologically. The cholera-bacilli were at times found to exist in the dejections of persons who had no external signs of illness, and in a few instances the comma-bacillus was not detected in the excreta or soiled linen of cholera-patients. Of the 336 persons attacked previous to December 14th, 185 or 55 per cent. died. The mortality among children was very marked, and it is pointed out that the consumption of unripe fruit and raw vegetables undoubtedly renders young persons less able to resist the germs than adults. The infection was much more virulent at the first outbreak of the cholera than during its later stages; thus in November, out of eight cases, five presented no clinical evidences of the disease. The district was largely inhabited by miners and colliers who went considerable distances from home to work, and the mine-owners joined with the local authorities in taking all the necessary steps to prevent infection. In many of the villages the drainage or pumpings from the mines furnished the water-supply. The police authorities were in direct communication with the sanitary department. Movable barrack-hospitals were erected in the infected centres; the provision varied from twenty-eight beds per 10,000 of the population to 122 beds per 10,000. A special bacteriological laboratory was established in the district, which is extremely populous, about 130,000 persons living on an area of 3½ Prussian square miles. Great difficulties were imposed upon the authorities by the enormous traffic and intercourse between the inhabitants of this Prussian border state and Austria and Russia, where at that time cholera was raging, and the disease was constantly being imported by smugglers, tramps and beggars. To combat the dangers entailed on this account, a special system of passes or travelling permits was introduced which operated for a 3-mile radius outside the frontier, and all passengers and goods traffic

on railways were subject to stringent surveillance. Fairs, pilgrimages, and every kind of public concourses were prohibited. All schools in the infected centres were closed. The rivers and streams were watched, bathing was forbidden, and boats and crews were disinfected. Receptacles had to be provided on board of all vessels to receive the excreta; nothing was to be thrown into the rivers. Disinfecting apparatus was procured, filters of good construction were supplied, and privies and middens were disinfected and emptied daily. All persons passing the frontier were subjected to strict medical inspection. Inns and lodging-houses frequented by tramps and beggars were systematically supervised. In any case where workpeople were attacked, all the fellow-workers were medically examined. People sleeping in the same house with those found to have the cholera were placed for the next five days under special surveillance. A service of medical men was established to look after cholera-patients and to see that disinfection was in every case thorough and complete. The Author concludes with a series of precautions found to be advisable or necessary in similar cases.

G. R. R.

The Purification of Sewage by Forced Aeration.

By G. E. Waring, M. Inst. C.E.

(Report on an Experimental Investigation.[1])

The object of these experiments was to find a way of artificially producing conditions more favourable to the bacterial destruction of the organic matter in sewage than can be found under ordinary circumstances. The process consists of the mechanical straining out of all such matters carried in suspension, and in their subsequent destruction by means of forced aeration and the purification of the clarified sewage by bacterial action in artificially aerated filters.

The sewage from the main outfall sewer of the City of Newport, R.I., was employed in the experiments. The city is sewered according to the combined system. The street inlets deliver into large catchment-basins, which in dry weather are little better than cesspools, and many of the sewer connections are merely overflows from old cesspools. The sewage contains practically no manufacturing wastes. The main sewer is 5 feet wide and 5 feet high, with a fall of 1 in 2,000; deposit and putrefaction are therefore constantly going on in it. A building 14 feet square was erected over one of the manholes, to contain a pump capable of lifting 90 gallons per hour, from near the invert of the sewer; and a 28-inch blower, which worked at about 3,000 revolutions per

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[1] The original is in the Library of the Inst. C.E.

minute, for aerating the tanks. The two together required 1·5 HP. to work them.

The sewage was delivered by the pump on one of two shallow beds of coarse broken stone, 8 inches deep and 20 square feet in area, which retained the coarser solids contained in the sewage. The impurities in the screens thrown out of action rapidly disappeared in the intervals of rest, but the area was too small to prevent the screens occasionally choking. The sewage next passed to the straining-tanks, each of 820 gallons capacity. These were cylindrical in shape, and each had a false bottom a few inches above the natural one, perforated with $\frac{3}{4}$-inch holes 4 inches apart. A six-branch from the air-pipe discharged into the space between these two. On the false bottom rested a 6-inch layer of broken stone, 1 inch to $2\frac{1}{2}$ inches gauge, upon which stood a cylindrical diaphragm of hooped staves, reaching above the top of the tank. The space inside and around these cylinders was filled in the four tanks with different sized stones, varying from that of the 6-inch layer in No. 1 to gravel $\frac{1}{8}$ inch diameter in No. 4. Each tank had a drain-cock near the bottom to take samples.

The sewage was delivered on one of these tanks inside the diaphragm, passing down through the material inside of it, and rising through that in the annular space between it and the tank, and was led thence to another of the tanks or to the aerator. Samples drawn from the cocks showed that the suspended matters were almost entirely deposited in the downward flow, when the sewage moved through these strainers at the rate of 3 feet per hour. The sewage was not improved by passing through more than one of the tanks. When a strainer was drained, air from the pipe rose through the tank, the bacteria of decomposition were stimulated into activity, and the organic matter was reduced to its mineral constituents; part of these being soluble passed away with the next flow of sewage, and part escaped in the gaseous form. No. 1 was found to be difficult to clean owing to the size of the interstices allowing large masses of sludge to accumulate.

The effluent then passed into an aerator made the same size as the strainers, also with the double bottom. The false bottom was covered with 6 inches of broken stone as before, and with smaller stone above to support the finer filling material of clean white gravel 3 feet 9 inches deep; on this was placed another 6 inches of broken stone, with finer stone above covered with a 6-inch layer of fine beach sand. The top of the tank was closed and two drain-pipes communicated through the cover to the upper layer of broken stone. There was no diaphragm. The effluent entered at the top, trickled down through the broken stone and gravel, and ran out at the bottom through a pipe which discharged at the bottom of an upright length of vitrified pipe closed at the lower end, thus preventing air from escaping with the effluent. In this tank the forced aeration was continuous, air being delivered at the bottom at a pressure of 2 to 4 inches of water, which passed up through the gravel to the upper layer of broken stone and

escaped through the drain-pipes. At first the sewage passed quickly through the tank, showing no signs of improvement; but as the surface of the sand clogged, it was distributed over a larger area until the whole surface of the tank was covered with liquid 2 to 3 inches deep, which secured uniformity of distribution throughout the tank. Gradually also the micro-organism of nitrification began to multiply, taking about thirteen days to get to the normal stage, when the organic dissolved impurities were decomposed into nitrites and nitrates, which escaped with the effluent.

Another tank, same as the last, but filled with coke instead of gravel, gave rather better results.

The average flow through the strainers was at the rate of 3,200,000 gallons per acre, and through the aerators 880,000 gallons per acre. The average percentage of purification, represented by the removal of organic nitrogenous matter, accomplished in the strainers was 51 per cent., and in the strainers and aerators together 92 per cent.

The experiments lasted five months, and when the tanks were taken to pieces the pebbles and gravel appeared as clean as when put in.

A. W. B.

Destructor-Furnace erected at the Quai de Javel, Paris.

By Georges Vitoux.

(La Revue Technique, September 10, 1895, p. 385.)

The average amount of refuse collected in Paris is estimated at 1,308,000 cubic yards annually, the quantity contributed varies as much as 30 per cent. from one month to another, in different districts, and the weight per cubic yard fluctuates also between somewhat wide limits, from 1,162 lbs. in summer to 1,477 lbs. in winter. The total for a population of 2,400,000 persons is about 570,000 tons; or approximately 530 lbs. per head per annum. It is pointed out that the duty of collecting this refuse is essentially one which should be entrusted to a public municipal body, with wide powers, and the various matters which should be insisted upon are specified. Until recently the refuse of Paris has been largely disposed of for agricultural purposes. Specially low railway rates were obtained, and a tariff of 2*s.* per ton, for distances not exceeding 93 miles round Paris, enabled the refuse to be distributed over a very large area. Analyses are given of several samples which contained nitrogen and phosphates valued at from 9*s.* to 7*s.* per ton, but the price obtained in the country was very low, averaging 7½*d.* to 1*s.* 3*d.* per ton on the truck, which with the carriage made the total price to the farmers from 2*s.* 10*d.* to 3*s.* 7*d.* per ton. Attention is called to the increasing use in England of furnaces for the destruction of all the combustible matters

contained in the refuse, and statistics are given of the number of cells of the different types of furnaces employed, by far the largest number being on Fryer's system, of which a detailed description is given. An account follows of a trial, which has been recently made at Paris, of this plan of getting rid of the refuse, and by references to photographs and diagrams, the apparatus of Messrs. Toisoul and Fradet, modelled upon that employed at Leeds, which has been made use of for this purpose, is described. The furnace is capable of dealing with from 2½ to 14 tons in the twenty-four hours, the average consumed being 5 tons. It is thought, however, that this plan is hardly a rational method of dealing with substances, which, used otherwise, possess a substantial value.

G. R. R.

The Plans for the partial Reclamation of the Zuyder Zee.

By F. Eiselen.

(Deutsche Bauzeitung, vol. xxix., September, 1895, p. 469.)

An account is given of previous projects for the recovery of land from the Zuyder Zee, and by reference to a map, the Author shows the accepted plan, as finally modified, after it had been reported upon by a government commission in April, 1894. In its main features, the project embraces the enclosure of the south part of this inland sea, by means of a vast dam stretching from the Island of Wieringen to Piaam on the Frisian mainland, and the formation of four polders round the shores, leaving the central portion, of about 560 square miles, still covered with water, which will in future constitute the so-called Yssel lake. The construction of the dam will be of course the most difficult part of the work, and its effect will be to convert the Zuyder Zee into a fresh-water sea. The presence of the outer dam, which has a total length of about 18·6 miles, will moreover enable the dams enclosing the polders, which extend to more than 100 miles in length, to be much lighter in section than they would have to be if situated in the open sea. A section of the outer or main dam is given, the depth of water in which it will have to be constructed varies from about 13 to 16 feet, and the bottom is principally sand. The portion of the Zuyder Zee not enclosed within this dam, consists for the most part of sandbanks, and would thus be of little agricultural value even if reclaimed. It was found, however, practically impossible to connect the fringe of islands surrounding the entrance to the Zuyder Zee on the north-west by means of dams, as the depth of water between them reaches 98 feet, even in one instance 131 feet. The commission have greatly increased the section of the dam, and its height is such as to exclude the waves, even in the case of the most severe storms recorded. The summit-level of the dam is, for reasons given, fixed at 18 feet 4 inches above datum at the

eastern end, and 17 feet at the western end; a mean height therefore of 17 feet 8 inches. This will involve the raising of the dam which now protects the Island of Wieringen to 17 feet, although its existing height of 13 feet 1 inch has never yet been invaded. The width of the summit of the dam is fixed at 6 feet 6 inches. On the side next the sea the slope is partly 7 to 1. The toe rests on a special subsidiary dam below high-water line. On the inner side there is a short slope of 2½ to 1, occupying 11 feet 6 inches, and then a berm of 55 feet 9 inches wide, which carries a roadway and two lines of rails; the slope on the inner side is at 3 to 1. The total width of the dam at the base is about 303 feet. Details are given of the mode of forming this dam, which will consist mainly of sand with a surface coating of clay.

There will be five groups of sluices, which will most likely be placed in a special canal on the eastern side of the Island of Wieringen. Each group will consist of six sluices, 32 feet 9 inches in width, the level of the sills being 14 feet 5 inches below datum. Westward of the sluices will be two locks, the extreme width of the larger one being fixed at 32 feet 9 inches to prevent the passage of battle-ships in time of war. Both locks and sluices will be constructed in special canals formed to receive them. An account is given of the precautions which will be taken to avoid the possibility of scour which might seriously affect the depth of the sea bottom when the dam approaches completion. An island will be formed 1,640 yards in length by 109 yards in width, about the centre of the dam, and the work will proceed east and west from the island, and also from either extremity, so that four points will be attacked at once. The town of Harlingen, which now does a considerable shipping trade with the southern end of the Zuyder Zee, will be shut out on the completion of the dam, but it will be provided with a ship canal 65 feet 6 inches in width at the bottom, and 7 feet 10 inches in depth, connecting it with the Yssel lake.

For all the works an expenditure is contemplated of upwards of £16,000,000, and before the land to be reclaimed comes into use, it is estimated that, with interest at 3½ per cent. and compound interest, the total outlay will reach £26,770,000. It seems doubtful whether the sale of the reclaimed land will recoup this expenditure, which would amount to over £55 per acre recovered. From the experience gained in other polders, however, it is thought that a rental of 41*s.* per acre might be reckoned upon, and this would represent a return of 3 per cent. on a capital outlay of over £32,300,000 sterling, a sum considerably in excess of the estimate. The time needed for carrying out the entire scheme is set down at thirty-three years.

G. R. R.

The Method of Two Distributed Loads for the Calculation of Stresses in Metal Bridges and Girders. By Ed. Collignon.

(Annales des Ponts et Chaussées, July, 1895, p. 5.)

The Author describes a method of calculating the stresses due to a moving load on a girder, by treating this load as consisting of two uniformly distributed loads, each occupying a portion of the girder. This method is applicable when—as is the case with the standard train-load in France—the moving load can be divided into a heavy portion and a light portion, each of which may be considered, without appreciable error, as a uniformly distributed load. The method is simplified when the heavy portion of the load is an even multiple of the light portion; thus, in the French train-load already referred to, the heavy portion, comprising the two locomotives, is twice the weight per foot run of the train-load following.

The Paper is divided into four chapters :—(i.) Bending moments on independent girders; (ii.) Bending moments on continuous girders; (iii.) Shearing stresses; and (iv.) Investigation of the elastic curve. The formulas for the bending moments and shearing stresses on independent girders are simple, and more or less accurate according as the assumed distributed loads approximate more or less closely to the actual loads. For the bending moments, the Author finds the "Eccentricity of the maximum moment,"—that is, the distance from the centre of the girder, of the point at which the maximum bending moment occurs—and gives the value of this maximum moment. A Table is also given showing the principal figures required for calculating the stresses due to the French moving load in girders from 98 feet to 492 feet span. Chapters (ii.) and (iv.), as might be expected, involve higher mathematics, and the formulas are more complicated. The letterpress is illustrated by several small explanatory diagrams.

R. B. M.

The Influence of Unequal Heating of a Girder with Redundant Supports. By Maurice Lévy.

(Le Génie Civil, September 28, 1895, p. 349.)

This is a short Paper on the stresses caused in a girder, with intermediate supports, by unequal heating of the upper and lower flanges; and the Author's attention was drawn to the subject by some observations of Mr. Deslandres with reference to the Bezons bridge. This bridge has continuous spans, and, in August, 1895, a difference of 13·8° C. was observed between the temperatures of the top and bottom flanges. Mr. Deslandres calculated that this difference increased the stresses on the metal by from 1·1 ton to 1·3 ton per square inch.

The Author is of opinion that this estimate is too low, and proceeds to consider the increase of stress due to a difference of temperature between the upper and lower flanges in the three cases of a girder (*a*) fixed at one end, (*b*) fixed at both ends, and (*c*) continuous over intermediate supports. He assumes in the first case that these stresses, due to variations of temperature, have a resultant passing through the free abutment, and, taking this abutment as origin, he obtains the equation:—

$$\int_0^l \left(\frac{M}{EI} - \frac{\delta\,\theta}{h}\right) x\,dx = 0.$$

Where

l = the span of the girder,
θ = difference of temperature between upper and lower booms,
δ = coefficient of extension of the metal,
h = the effective depth of the girder at point distant x from origin,

M, E and I having the usual interpretations. He obtains similar equations for the other two cases, and concludes that, in the case of the Bezons bridge, the stresses were, in reality, increased by from 1·6 ton to 1·9 ton per square inch. The Author is therefore of opinion that where appreciable differences of temperature can occur between the upper and lower members of girders, independent spans should be used in place of continuous ones, on account of the heavy stresses to which the latter are subjected by this unequal heating.

R. B. M.

Temporary Railway-Bridge over the Mandau. By R. Muller.

(Der Civilingenieur, 1895, p. 273.)

The narrow gauge Zittau-Oylin-Jonsdorfer Railway, built during the years 1889–90, crosses the valley of the Mandau not far from the town Zittau. The wide valley is annually flooded, sometimes to a height of 6½ feet, the river-bed being so winding that the flood-water is not carried away quickly enough. At the time of building the railway a scheme for the regulation of the river was under consideration, so that as the position of the future river-bed was not definitely fixed, it was resolved to carry the railway across the valley by a temporary bridge.

Since railway-bridges are now seldom constructed of wood, and since the temporary bridge will ultimately be taken down, it seemed worth while to place on record some details as to the design and execution of the work.

The total length of the bridge is 287 feet, the river being crossed by three spans of 26 feet, and the left bank being divided into

fourteen spans of 13 feet each. On the left bank single beams are used, while double beams are at the long spans over the river. Each pier contains four piles, the two outer piles being placed at an inclination to the vertical of 1 in 5. The three piers in the river-bed are protected by ice-breakers.

Two pile-drivers were used during the work. The total cost of the bridge was £425.

The Paper is accompanied by two sheets of drawings.

A. S.

Filling-in Railway-Trestles with Sluiced Material.

(Railway Review, Chicago, September 21st, 1895, p. 524.)

The Northern Pacific Railroad is giving a great deal of attention to its roadbed and track both in connection with the trestles and with gradients. The work of making permanent all bridges, as far as possible, which has been in progress during the past two years, has been continued. Forty-two miles of trestle-bridges have been disposed of by filling, whereby a permanent embankment has been secured, with the result of very greatly decreasing operating expenses. Another reason for the filling-in of trestles is that by this means the serious danger of accident from fire is averted. It is intended to fill in all the timber trestles in the Cascade mountains on both sides of the summit tunnel, and five of them have already been completed. The work is being done by Mr. E. H. McHenry, chief engineer of the Northern Pacific Railroad, by the sluicing process. The filling-material is loosened by the stream from a powerful hydraulic jet and is conveyed to the trestle in flumes on the hillside beyond the trestle. The requisite water is obtained by the diversion of one of the numerous mountain streams which may be found at frequent intervals all through the Cascade range. In the case of the particular bridge illustrated in the article, the sluiced material was conveyed a distance of half a mile from the borrow-pit, and the cost per cubic yard was but five cents, including all charges. The process of excavating and moving material by hydraulic jet and flumes is not new, but it is believed that the method of building levees, or banks, and maintaining a perfect and uniform slope is entirely novel. It has been found that hay or straw perfectly serves the purpose of a binder in constructing these levees, and a slip or washout is of very rare occurrence. The straw and washed material are deposited in alternate layers of 6 inches, and the embankment is self-puddling.

The top of the levee is usually maintained at a height 1 foot above the material brought down by the flume, and sometimes boards are used in the inside face to prevent washing, but this is not strictly necessary. When used, the boards are drawn up as the filling rises. Short sections of plank with braces attached are

used for deflecting the current to any desired point. The bank, when completed, is thoroughly compacted, and no settlement is found to occur. As already stated, five trestles have been filled in this way, at a cost of from five to eight cents per cubic yard, and it is expected that this method will be employed for all of the trestles upon both sides of the summit. The cost of the embankments is considerably less than that of rebuilding the trestles, which is another favourable recommendation of the method, beside that of the permanent results obtained.

The Prevention of Noise caused by Trains in passing over Iron Railway-Bridges. By — Boedecker.

(Verhandlungen des Vereins für Eisenbahnkunde, 1894, p. 125.)

On the Berlin-Potsdam Railway there are a number of iron bridges of similar construction carrying the railway over important streets in Berlin. These bridges are of the plate-girder type, with cross-girders spaced apart at a distance equal to the ordinary interval between sleepers; the flat-bottomed rails rest on bed-plates, and are bolted through to the top flange of the cross-girders; the decking between the girders consisted of corrugated sheet-iron. The noise caused by trains in running over these bridges was so great, and the public complained to such an extent, that the Author was instructed to devise some means of diminishing it.

The first method tried was to replace the corrugated-iron decking by one consisting of two layers of planks, between which a watertight layer of felt was laid. This did not prove satisfactory, as the noise was not sensibly reduced, and water leaked through between the rails and planks. The second method consisted in laying Haarmann's sleeper-rails over the bridges, the rails resting on timber packings on the cross-girders, with layers of felt between the rails and packings; by this means the extra noise at the rail-joints disappeared, but the whole noise of the train passing over the bridge was only diminished a little, and the result was still not satisfactory.

The third and final method consists of a decking of 1¼-inch planks between the cross-girders, resting on 3-inch timbers laid on the bottom flanges. On the planks a double layer of felt (Papplage) is laid, which is fixed to the vertical web of the cross-girder; at the connections with the girder a timber cover-joint is placed on the felt, and two hooked bolts connect the whole firmly to the bottom flange of the cross-girder. A depth of 4 inches of slag gravel covers the decking, and the decking is inclined towards the centre of the bridge, small tubes being inserted through the decking connected to a suspended gutter for drainage purposes; great care is taken that the felt is fixed accurately to the girders and drainage tubes, and an arrangement is made by means of which

only water and no gravel can pass through into the gutter. A layer of felt is also laid between the planks and the timbers they rest upon, and the ironwork in contact with the decking and ballast is well asphalted. The whole decking weighs 600 lbs. per lineal yard for a single-line bridge, 11 feet in width, and costs 11·15*d.* per square foot.

The result of this means for preventing noise has been so satisfactory that other bridges are being treated in the same manner; the decking has moreover proved watertight.

A plate containing sections of the decking, girders and rails is attached to the Paper.

J. A. T.

The Influence of Braked Trains on the Superstructure of Metallic Bridges. By F. JASINSKI.

(Revue générale des Chemins de fer, July, 1895, p. 22.)

The Author states that the necessity of taking into account, in the calculations of the resistance and stability of large bridges, the forces produced by the passage of a train with the brakes on has already been pointed out, and the longitudinal force acting on the rails has been estimated, as well as the secondary forces produced in the main girders; but up to the present no attention has been given to the influence of braked trains on the superstructure of bridges, the ordinary construction of which gives a very insufficient resistance in this respect. Taking into consideration the fact that continuous brakes are being used to a great extent, and that on certain railways all the wheels of express trains are provided with brakes, this question has become of great importance.

The increased resistance which the rail offers to the movement of a wheel when it is braked is not greater than the adhesion f Q, f representing the coefficient of friction, and Q the load on the wheel. The limit of this adhesion may be taken as $F = 0 \cdot 25\,Q$, in which F represents the tangential reaction of the rail against movement of the wheel. The rail is exposed to an equal and inverse force from the wheel, which tends to cause it to slide in the direction of the train, this force being transmitted to the bridge partly by the surfaces of contact, and partly by the fish-plates or other arrangements for preventing the sliding of the rails. It follows, then, that in bridges of considerable spans, the tangential force of a braked train can be entirely transmitted by the superstructure of the bridge to the main girders, and from the latter to the supports. The Author proceeds to discuss the question as to whether the construction of railway bridges satisfies this condition, in so far as the superstructure is concerned, leaving aside the question of the effect of braked trains on the general

stability of bridges and the secondary tensions produced in the main girders.

In the ordinary superstructure, consisting of cross-girders and rail-bearers, a kind of frame is formed, which is composed of three rows of rectangles in the case of single-line bridges, or five rows with double-line bridges. The rectangles under the lines of way are often strengthened by special bracing. With this construction the longitudinal forces are transmitted to the main girders by the rail-bearers and cross-girders. All the cross-girders having the same dimensions and being fixed in the same manner to the main girders, it may be assumed that they receive the same longitudinal forces acting on the bay during the passage of a braked train.

The deformation of the rail-bearers is neglected on account of its being very small. If p is the weight of a train per metre, d the distance between the cross-girders, the Author finds that the longitudinal horizontal force q acting on each cross-girder at the connections with the rail-bearers will be $q = \frac{1}{2} f p d$. These forces produce corresponding re-actions in the main girders, and tend to cause the cross-girders to deflect in the horizontal plane independently of the vertical deflection caused by the weight of the train. Besides these horizontal and vertical flexures, the cross-girders will be subjected to torsion, due to the fact that the forces q are not applied in the same horizontal plane as the re-actions of the main girders.

Taking as an example a Russian railway bridge, the Author calculates the stresses produced in the flanges of the cross-girders by the passage of a train, all the wheels of which are braked.

The superstructure of this bridge is constructed independently of the main bracing. The principal dimensions are as follows:—

		Feet.
Length of the cross-girders	=	18
Distance between the cross-girders	=	16
" " rail-bearers	=	6

The weight p of an express passenger-train being taken as 0·46 ton per lineal foot, the horizontal force acting on the cross-girders at the points of connection with the rail-bearers will be—

$$q = \frac{1}{2} \times 0 \cdot 25 \times 0 \cdot 46 \times 16 = 0 \cdot 92 \text{ ton.}$$

Proceeding, the Author finds by calculation that there is a maximum total strain of 6·73 tons per square inch in the extreme edges of the flanges of the cross-girders.

Such a strain is very considerable, especially in taking account of the other secondary tensions to which the cross-girders are subjected, together with the dynamic force of the shocks which are always present in the movement of a braked train. The Author states that this example sufficiently denotes the serious influence of the passage of braked trains on the superstructure of ...nd points to the necessity of strengthening them, before

exposing them to the passage of trains having all the wheels braked.

A sketch is given of a single-line bridge showing what the Author considers is one of the surest, and at the same time, the simplest and least costly method of bracing in which to obtain the strengthening required. It consists in having a tie beam—an angle bar 3 inches $\times$ 3 inches $\times \frac{5}{16}$ inches—from each connection of the cross-girder with the main-girder to the centre of the adjacent rail-bearer, the two rail-bearers having an independent bracing between them; the dimensions of the angle-bars mentioned are, in the Author's opinion, quite sufficient to resist the longitudinal forces produced.

In the case where the cross-girders are fixed to the gussets of the main girders between the flanges, the difficulty of strengthening the superstructure to resist these forces is very much increased. The Author suggests that in the case of main girders of considerable depth, the gussets to which the cross-girders are attached could be strengthened by means of struts, one of the ends of which would be riveted to the gusset, at the height of the neutral axis of the cross-girder, and the other to the foot of the next gusset; and also for the same purpose, connect all the gussets together by a horizontal bar, of a length equal to the length of the main girder, and fixed at the height of the neutral axes of the cross-girders.

J. A. T.

The Railways of Greece. By — Schwering.

(Verhandlungen des Vereins für Eisenbahnkunde, Berlin, 1895, p. 4.)

This Paper opens with a general description of the country, showing the topographical and geological features, the agricultural and industrial conditions prevailing, the statistics of the shipping, exports and imports, and the amount of the public debt. From the nature and amount of these is to be seen that the country does not offer a favourable field for railway enterprise. The construction of lines in the interior is attended with great difficulty owing to the mountainous character of the land. Where the topography is more favourable, as, for instance, along the Peloponnesian coast, the economic development of the lines has to suffer from the competition of sea-carriage. The Author then proceeds to a description of the different railway systems of the country.

The "Piræus-Athens" was the first line constructed. It is 5·6 miles long, and was built in 1862 by an English syndicate, and afterwards taken over by a Greek company. The present capital of the line amounts to £200,000. The gauge is 4 feet 8½ inches; the maximum gradient 1 in 55·5, the minimum radius of curves 25 chains. The traffic over the line, which is a double one, is considerable, in spite of the competition of a steam-tramway running as far as Phaleron. An extension of the railway has

recently been made so as to bring the terminus, which was formerly on the outskirts of Athens, into the heart of the city. Up to last year, however, this extension had not been opened, owing to differences between the contractors and the company.

The financial condition of the line is superior to that of any other in Greece. The gross earnings per mile in 1892 amounted to £8,990, the expenses to £3,071. The concession terminates in 1937.

The next line from Pyrgos to Katakolo was built in 1881. It has a length of 9·3 miles, and is of metre gauge, with gradients up to 1 in 40, and curves of 500 feet minimum radius.

In 1882 an energetic development of railway and road work construction took place. For the purpose of studying the question and carrying out the work, a number of engineers were obtained by the Government from France; but only a portion of the contemplated works were executed, owing to the want of funds. In the same year concessions were also given for a number of private railways. Amongst them were the lines of the Peloponnesus and Thessaly. These were all to be of metre gauge as being more adapted to the requirement of the topography and financial conditions of the country. The only exception made was in the case of the Piræus and Larissa Railway. The Railways of Attica, 46 miles in length, with maximum gradients of 1 in [illegible] and curves of 360 feet minimum radius connect Athens and Laurium, a branch running to Cephisia. The share capital is £216,000. In 1892 the earnings amounted to £600, and the expenses to £290 per mile. The concession runs to 1981.

The "Peloponnesian Railways."—Concessions for the following lines were obtained by the "Compagnie des chemins de fer Piræus-Athènes-Peloponèse."

—	Length in Miles.	Date of Concession
1. Piræus, Patras and Pyrgos	205	1882
2. Pyrgos and Olympia	13	1890
3. Corinth and Nauplia	40	1882
4. Argos and Myli	6	1882
5. Karasila and Cyllene	10	1882
6. Vartholomo and Lintzi	7	1882
7. Myli and Tripolitza	36	1887
8. Kalamata, Diavolitzi and Nissi	26	1887
Total	343	

The Piræus and Pyrgos Railway passes mainly through fertile lands bordering on the Peloponnesian coast, and offered only few engineering difficulties. It crosses the Corinth canal by an iron bridge at an elevation of 164 feet. The summit of the line lies between Athens and Eleusis, 564 feet above sea-level. The maximum gradient is 1 in 40, the sharpest curve 360 feet radius.

The Pyrgos and Olympia line passes through hilly country, and attains an elevation of 223 feet.

The Corinth and Nauplia branch rises with maximum gradients of 1 in 40 to a height of 1,050 feet.

The Myli and Alamata Railway, 112 miles long, has been the cause of a great deal of trouble. A concession for its construction and working was granted to a Belgian company in 1887. The estimated cost amounted to close on a million sterling. Work was begun in 1888, and came to a standstill in 1891, when the concession was forfeited owing to the company not being in a position to carry out the contract. The difficulties of construction appear in this, as in some other cases, to have been underestimated. They are due, apart from the obstacles caused by the topography of the country, to the fact that, owing to the great heat, work can only be carried on during the months from September to May, and that for the execution of the greater part of this the introduction of foreign labour is necessary. After the forfeiture of the concession, it was handed over, with an additional subsidy of £120,000, to the French company previously mentioned. This contract, too, proved unsatisfactory, as at the present day only the two ends of the line have been opened for traffic, work on the central section being entirely abandoned.

The branch line (2 feet 6 inches gauge) connecting Diakophto with Kalavryto also proved a source of trouble to the Government. To attain the elevation (2,460 feet) of Kalavryto, it was found necessary to build a part of the line as a rack-railway, with maximum gradients of 1 in 6·9 and curves of 262 feet minimum radius. The respective amounts of those on the remainder of the line are 1 in 28 and 115 feet. The actual cost of the 13 miles of this railway was £9,200 per mile, which is considered to be low when the nature of the country traversed is taken into account. The line had to be carried through numerous galleries and tunnels cut in the almost vertical sides of mountain gorges which required frequent bridging. The construction of railways of this character, the Author considers, is not warranted by the conditions of the country.

The Peloponnesian Railway Company, with a capital of £1,200,000, is at present in financial difficulties. This is mainly owing to the depreciation of the Greek currency, as the ratio between working expenses and earnings does not exceed 60 per cent.

Towards completing the Peloponnesian system the following concessions have been granted:—

Olympia to Karitena, 34 miles; Xyrocambi to Githion, 22 miles; Karitena to Leondari, 12 miles; Pyrgos to Meligala, 86 miles; Leondari to Xyrocambi, 38 miles. All the lines to be of metre gauge. Preliminary surveys are also being made for a line of 2 feet 6 inches gauge from Kalavryto to Tripoli. Whilst the present financial crisis continues there appears, however, but little prospect for the construction of any of these.

The railways of Thessaly, for which, in 1882, a concession, together with a subsidy of £1,287 per mile was granted, are in a more flourishing condition, mainly owing to the more productive nature of the country traversed, although there is still considerable room for an improved and more extensive cultivation of the soil, only a third of the fertile land being at present under the plough.

Volo, at the head of the gulf of the same name, was chosen as the starting point of the system of 126 miles; its population having increased from 4,000 in 1881 to 11,000 at the present date, whilst that of Larissa is rapidly decreasing. The line runs in a general westerly direction to Kalabaka, passing through Valestino, from which point a branch leads to Larissa. On the latter are few difficulties, but the construction of the line to Kalabaka, which crosses the Kara Dagh range at an elevation of 771 feet, entailed the introduction of gradients up to 1 in 33 and curves of 492 feet radius. The number of bridges and openings in a distance of 45 miles amounts to 237, with an aggregate length of span measuring 3,860 feet. The working expenses amount to 52 per cent. of the gross earnings, and suffice to pay a small return on the capital invested. The average speed of the passenger trains is about 22 miles per hour. The construction and maintenance of the line are stated to be remarkably good.

Of the lines completed since 1882 it only remains to mention the Missolonghi to Krioneri and the Missolonghi to Agrinion. The former was built by a private company, the latter by the Government. The gradients do not exceed 1 in 50, and the curves are nowhere sharper than 393 feet radius. The receipts of both these lines have, so far, been only very moderate.

Piræus and Larissa Railway.—Whilst the foregoing lines are mainly of importance for opening up the country, the Piræus and Larissa railway, when completed, may become of importance for international traffic. If, which is yet by no means certain, the extension of the line from Larissa, to form a connection with the Turkish railway system, should be successfully negotiated, and when the line to Larissa shall have been completed, Piræus will be the nearest to Port Said of all the ports connected with the European railway system. Should this railway communication take place it is surmised that Piræus would become the port of call of the mail steamers for the east in place of Brindisi or Naples as at present. The distance between Brindisi and Port Said is about 1,050 miles, whilst that between Piræus and Port Said amounts to about 700 miles—a saving of 350 miles. The actual saving in time, the Author claims, from Port Said to Berlin would amount to twenty hours; to Vienna, twenty-four hours; to Brussels, fourteen hours; to Calais, eight hours. With the object of making the line suitable for international traffic it was built of standard gauge.

An extended description is given of the location as laid down by the French engineers in the employ of the Greek Government. In 1889 a loan of £3,200,000 was raised, and tenders invited for the

construction of the line. The contract was awarded to an English firm at £9,012 per mile, for the distance of 214 miles from Piræus to Larissa. The branches to Stilida and Chalkis, having an aggregate length of 28 miles, were let at £5,793 per mile. These prices included the working surveys and the rolling stock. The land was obtained by the Government and handed over to the contractors. According to the topography the maximum grades of the different sections were not to exceed 0·011 to 0·20[1] (1 in 91 to 1 in 5), and the minimum radii of the curves 1,640 to 984 feet. The weight of the rails to be 68 lbs. per yard.

The final location as made by the contractors only approximated in places to that of the preliminary laid down by the French engineers. In order to avoid nearly 2 miles of tunnel through the Cithæron range, the direct route from Athens to Thebes was abandoned, and a more westerly[2] course taken towards Marathon. By means of an ascent of 1 in 62·5 and two short tunnels the northern slope of the Parnes ranges is attained at an elevation of 1,150 feet, after which the line descends, by way of Malakassa, to Thebes (62 miles). From this point the route traverses the Bœotian plains, skirts the borders of Lake Copais, and, passing a short distance from Levadia, reaches Dadi, after which the ascent is made of the Oeta range, which is pierced at an elevation of 1,340 feet by a tunnel 2,295 yards long. From here the line is carried by way of a formidable gorge across the Lamia plains to Lianokladi, where a junction is formed with the branch to Stilida (11½ miles). The section on this side of the summit presents unusual difficulties owing to the nature of the sides of the Bralo gorge, which are remarkably steep, and in many places liable to slips. In the distance of 13 miles there occur over 2,400 yards of tunnel (excluding the summit tunnel), and numerous viaducts of considerable dimensions. One of these has a length of 984 feet and height of 197 feet; and is estimated to cost £64,000. From Lianokladi the line is carried over Styrfaka through the Voidorewna gorge and across the Othrys, at an elevation of 1,919 feet, by a tunnel some 500 yards in length. From here the line passes to the west of Lake Doukli (Xymia) instead of to the east as projected by the French engineers, after which it enters the plains of Thessaly.

The 8-mile section in the Othynis mountains comprises some heavy work, embracing about 4,600 yards of tunnel and several large viaducts. The construction of the line was commenced in the autumn of 1890, and in the early part of 1893 considerable progress had been made, the state of the works being as follows:— The first 12 miles out of Piræus, including permanent way, but not the station buildings, was completed. From here to Thebes the earthworks and bridges were well advanced; and on the branch to Chalkis those were finished. With the exception of the

[1] *Sic* in original; 0·020 (1 in 50) is evidently meant.
[2] *Sic* in original; this should read "easterly."

rock cutting between Levadia and Bralo, the greater part of the earthwork from Thebes to Levadia was done. Only about 600 lineal yards of the Bralo tunnel had been taken out. The most difficult part from Bralo to Lianokladi was in a backward state, and the larger viaducts were not commenced. The branch from Lianokladi to Stilida was in the main finished. Between Lianokladi and Larissa only a small part of the earth- and bridge-work on the first section was completed.

In 1892 differences arose between the Government and the contractors, which led to the almost entire cessation of work. On the intervention of England, an arbitrator, in the person of the Author, was appointed by the German Government to adjudge the points of issue. The decision was, on the chief allegations, in favour of the Greek Government. A point in dispute was a claim of the contractors of £120,000. This was disallowed by the Author, who decided that the constructors had been overpaid to the amount of £16,000. After this all work was stopped by the contractors. The Government contemplates handing over the line as far, for the present, as Thebes, to a new company which is to be formed for the purpose of completing and working it.

The Author deplores the fact of works of such magnitude, on which over a million sterling has been spent, lying idle and going to ruin, but states that with the disappearance of the present financial crisis, the country may be expected to carry to completion the works already begun.

A map showing the various lines accompanies the article.

J. R. B.

The Basle Street Railways. By O. Löwit.

(Schweizerische Bauzeitung, 1895, p. 28 *et seq.* 15 Figs.)

The Swiss Railway Department had asked for a concession to construct street railways in Basle, but the council of the Canton decided on the 31st March, 1892, to oppose the demand and to take up the work itself; permission to do this was granted on the 28th March, 1893, by the Swiss Government. The first section of the line from the central station to the Baden station was opened for work on the 6th May, 1895. The chief streets are first to be supplied, and the profit arising therefrom is to be expended on providing lines in the less frequented districts. The principal requirements were a six-minute service, which of course settled the positions of the crossings, but wherever possible the line was to be double.

A space of 8 feet $2\frac{1}{2}$ inches was fixed as the minimum between the roadside of a car and the opposite kerb-stone. The breadth of a car is 6 feet $6\frac{3}{4}$ inches, and the minimum distance between the body and the near kerb-stone was fixed at $9\frac{7}{8}$ inches, the distance from kerb to centre of track would therefore be 4 feet $1\frac{1}{4}$ inch.

Where two tracks are used the distance between centres is 8 feet 2½ inches, and the free space between passing cars is 1 foot 7¾ inches.

Double tracks are laid where a space of 8 feet 2½ inches can be obtained on one side, and where this can be obtained on each side the tracks are laid centrally. On the old Rhine bridge, the tracks runs close to the kerb, and some of the streets have a breadth of only from 9·8 to 11·5 feet. The Author gives a list of streets through which the line runs, and it appears that its total length is equivalent to 5,850 yards of single track. The space between the car and footway is generally 9·8 feet, and in the narrowest place 8·15 feet. The minimum radius is 49 feet and this only occurs once on the line; the total length of line is 3,040 yards, of which 2,420 yards, or 79·7 per cent., is straight and 610 yards, or 20·3 per cent., on curves. The rail-level follows the curve of the surface of the street, so that in one place the inner rail on a curve is 1½ inch higher than the outer rail, but this has so far given no trouble. Some of the grades are very unfavourable; the maximum grade is 5·2 per cent. for a length of 55 yards on the Steinenberg, which is reached by a grade of 4 per cent. for 58 yards on one side and 4·8 per cent. for 82 yards on the other. The highest part of the line is 114 feet above Rhine water-level, and the lowest part is 25·5 feet above it, the difference is therefore 88·5 feet.

The gauge between rails is 1 metre, the width used for all new lines; and the part from the Central Station to the Baden Station is built on the Phœnix solid-system. The rails are 5·5 inches high by 4·9 inches wide at the base, and the weight is 67·5 lbs. per yard. No sleepers are used, tie-rods 8·2 feet apart keep the rails to gauge. Each rail is 32·8 feet long, and all curved pieces were delivered shaped direct from the works to facilitate laying. In wood-paved streets the rails are laid direct upon concrete, while in stone-paved and macadam roads they are laid upon low dry walls. The old Rhine bridge had to be strengthened with beams and tie-rods, and this work was expensive, as there are thirteen arches. Cast steel and built-up points are both in use, in order to see which last best. The most important point is of course the choice of the power system, for upon this mainly depends the success of the line.

It was advisable to have a system with as little over-head work as possible, so the stirrup-collecting system of Messrs. Siemens and Halske was selected. This system differs from that in use in the United States, for in the former case a wide stirrup-shaped collector is used, in the latter a trolley. A sectional view of the collector is given in the original, from which it appears that it consists of a U-shaped rod of aluminium, having the inner space filled with hard fat. This gear causes hardly any wear on the line while affording good conductivity. The collector is 4·9 feet wide, and is slightly curved above so as to pass under the cross wires and points easily. The Author considers the system far preferable to the trolley. The conductor is supported upon cross

amental cast-iron brackets on the
wires fixed where possible to mpossible they are attached to
buildings, and where this is wires are as light as possible,
Mannesmann tubular posts. T en Station line, which chiefly
and on the Central Station—B ductors are carried along the
consists of two tracks the two co
single track part to avoid soldering. tor average about 38 yards

The points of support of the conduc where there are spans of
apart, except upon the Rhine bridge, divided into four parts
109 yards and 98 yards, the whole line is from lightning. The
insulated from each other and protected ted at the ends and
return is through the rails, which are conne
also across so as to afford good conductivity. tment required,

Besides this the Government Telegraph Depa circuits, the
for the protection of the telephone and telegrap eter, which
addition of uninsulated copper wire of 0·315 inch dia tervals of
is laid between the rails and connected with them at d water-
109 yards. This wire also affords protection to the gas- has put
pipes. As a further precaution the Telegraph Departme ductor,
a fuse into each telegraph wire which crosses the line c urbance
and this was found to act perfectly when tested. No dis power
of telephone circuits has yet been noticed. The site for t placed
station was not large enough to add a car-house, so this wa
270 yards away, and a siding joins the two places. ation.

The Author describes the arrangements of the power s each
The boiler-house contains two mild-steel Cornish boilers long
with three Galloway tubes. The shells are 28 feet 6 inch The
and 5·4 feet in diameter. Feed-water heaters are used. steam-
fuel is coke from the town gasworks. At present two of the
engines and two dynamos are in use. Each engine is of feet
horizontal compound type, with a heavy fly-wheel 16·4 with
diameter, cylinders 13·8 inches and 21·5 inches diameter ute;
29·5 inches stroke, and they run at 80 revolutions per min per
they are worked condensing with 135 lbs. initial pressure cut-
square inch. Each develops at 14 per cent. and 29 per cent.
off 92 and 130 I.HP. respectively, or about 75 and 110 B.HP. und

Each dynamo is driven by belting, and is of the shunt-wo or
direct-current type, developing 520 volts and 140 amperes ard
72,800 watts, at 450 revolutions per minute. The switchbo na-
appears to be of the ordinary type. In the workshop is a dy lts
motor which transforms the potential from 500 volts to 120 vo ch
for lighting purposes. A small accumulator is also in use, whi nd
is charged during the day. The wagon-shed has a basement, an
the ground floor is supported on girders, so that access is quite
easy to the underside of the cars. The size and other details are
given by the Author.

There are at present twelve cars, of which seven are in use on week days, while on Sundays two cars are run together, and all are in use. Each car has room for three persons at the front and five at the back, all standing, and sixteen sitting inside, making a total of twenty-four passengers. The length of the cars is 23 feet

6 inches over buffers, and other details are given, each car weighs 5 tons unloaded and 7 tons loaded, and is provided with a 15-HP. motor, which rests upon india-rubber cushions fixed to a frame carried on the four axle-boxes. The motor is of the two-pole type with drum armature and carbon brushes; it runs at 360 revolutions per minute, and the power is transmitted to both axles by a single-chain gear. These chains act well and do not stretch. In Mulhouse, where the same type of gear is used, it was not found necessary to shorten the chains after eleven months' work.

The direction of motion of the car is altered by merely switching over the current as the collector changes position automatically. A hand-lever brake is used, and can be supplemented in case of danger by sending a reverse current through the armature. The line was opened for traffic on 6th May, 1895, and at present a six-minute service is used, but it will be replaced by a three-minute service. The speed in wide streets is 9·3 miles per hour, and in narrow streets 7·4 miles per hour. With ten cars, on Whit Sunday 13,575 passengers were carried, and on Whit Monday 13,639 passengers, exclusive of 500 season-ticket holders. The cost of working is 6*d.* per car-mile, exclusive of depreciation, allowance, or interest on capital.

The Author states that the actual constructional work is to be given up by the town, the mechanical and electrical work and car building will be taken up by the Alioth Electrical Company of Mönchenstein, and by Messrs. Siemens and Halske of Berlin, and these firms have agreed to purchase the steam-plant from Messrs. Sulzer Brothers of Winterthur, and the car bodies from the Swiss Engineering Company of Menhausen.

E. R. D.

Rail-lifters used on the Eastern of France Railway.

By — Freund.

(Revue générale des Chemins de fer, June, 1895, p. 364.)

The Author states that after numerous tests, the Eastern Company have adopted a rail-lifter, in lieu of the ordinary lever, which is made by the "Société Alsacienne," the power of which is 3 tons, and the weight 66 lbs. The base of the lifter is 11·81 inches by 8·66 inches; the height of the box containing the screw is 19·68 inches, and the width and depth are 5·91 inches and 3·66 inches respectively. The screw is turned by an ordinary handle, the arm of which is 7·48 inches long. One of the main motives for the adoption of the rail-lifters is the fact that in the use of the ordinary lever two or three men are required during the whole operation of packing to maintain it in position; the length of the latter is also a great inconvenience.

According to comparative statements made by the Eastern

Company, the economies realized by the use of the rail-lifter in question are as follows: it is supposed that each gang of platelayers has two levers or two rail-lifters, which are employed in maintenance work one hundred and fifty days per year; the number of hours taken each day in a gang of five to seven men in rail-lifting is five with the ordinary levers, and two and a half with the rail-lifters; the cost of a lever is 12*s.* 11*d.*, that of a rail-lifter £2 19*s.* 6*d.*; the average cost of each for repairs per year are 4½*d.* and 8*s.* respectively, and the following Table is then given:—

——	Levers.			Rail-lifters.		
Number of hours employed for leverage—	£	*s.*	*d.*	£	*s.*	*d.*
5 hours × 150 × 2·88*d.*	9	0	0		..	
2½ hours × 150 × 2·88*d.*		..		4	10	0
Annual depreciation of two appliances	0	2	0½	0	9	2½
Annual repairs for two appliances	0	0	9	0	16	0
Totals	9	2	9½	5	15	2½

Difference in favour of the rail-lifter, £3 7*s.* 7*d.*

In a gang of six men, the difference in favour of the rail-lifter per man per year is $\frac{£3\ 7s.\ 7d.}{6} = 11s.\ 3{\cdot}16d.$

The Author is of opinion that this amount may be doubled if notice is taken of all the facilities which are produced by the general use of rail-lifters.

A sketch of the rail-lifter used is given.

J. A. T.

Coal-Dust Fuel.[1] By C. Schneider.

(Mittheilungen aus der Praxis des Dampfkessel- und Dampfmaschinen-Betriebes, 1895, pp. 336 *et seq.*)

In a Paper read before the Prussian Union of Boiler Inspectors, the Author gave a short historical account of the use of coal-dust as fuel for steam-boilers. After describing in detail the Wegener, Schwartzkopf, Friedelberg, Ruhl, and De Camp systems for burning coal-dust, he gave some details of trials of the Wegener system which he carried out at the request of Messrs. Wegener and Schmidt.

The boiler experimented on consisted of a lower shell, 6 feet

[1] See Mr. T. R. Crampton's Paper "On the Combustion of Powdered Fuel, &c." Journal of the Iron and Steel Institute, 1873, p. 91.—Sec. Inst. C.E.

3 inches diameter and 14 feet 6 inches long, with two internal flues 28 inches diameter, and an upper shell, 6 feet 3 inches diameter and 12 feet long, and containing seventy-two tubes 3½ inches diameter. The upper and lower shells were connected by two necks, 21 inches diameter. The working-pressure was seven atmospheres.

The coal-dust, thoroughly mixed with air, is introduced into the end of each of the flues and there burnt; the hot gases then traverse the internal flues, the outer surface of the lower shell from back to front, the external heating surface of the upper shell from back to front, and lastly, the seventy-two tubes of the upper portion. Five different trials were made on this boiler.

Two further trials were made with the Wegener apparatus fixed on a boiler of the locomotive type, shell diameter 6 feet 8 inches, length 18 feet, with ninety-six tubes 2¾ inches diameter. The working pressure was six atmospheres.

For the trials on the double boiler three kinds of coal-dust—Oberschleswig, English and Bohemian—with calorific values of 6,626, 6,516 and 5,264 heat-units respectively, were used. The analyses of the different coal-dusts is given. In the double boiler, the quantities of heat utilized were respectively 78, 79 and 78 per cent. of the total heat-values of the fuel; the steam production in four of the trials being considerably above the average with ordinary coal firing. The efficiency would have been still higher if losses through leakage of air into the flues could have been avoided.

The results of the trials on the locomotive boiler were not so favourable, the efficiency being on the first trial 70·6 per cent., on the second trial (during which the boiler was not pressed so hard) 73·6 per cent. The composition and temperature of the waste gases were observed at intervals. The percentage of carbonic acid in each of the five trials on the boiler did not vary much. The excess of air used in the first five trials was 0·84 per cent.

An analysis of the waste heat is given. In the first five trials it was evident that the ashes contained a considerable proportion of unburnt coal-dust; in the sixth trial, therefore, the ashes were analysed, from which it appeared that the heat lost due to imperfect combustion was 4·7 per cent. Loss by radiation and conduction varied during the trials from 5·8 to 16·8 per cent.; the greatest occurred with the locomotive boiler, which was not properly covered with non-conducting material.

The results of these trials, as compared with those got from the usual methods of burning solid fuel are very satisfactory, and it is hoped that by enlarging the combustion space more favourable results may yet be obtained. One disadvantage of the Wegener system lies in the fact that the coal-dust does not burn steadily, but in puffs succeeding each other at short intervals, so that a noise like the exhaust of a steam-engine is made. The inventors hope, however, to overcome these difficulties.

In June, 1894, three trials of the Schwartzkopf system were

made on an experimental boiler belonging to Mr. Richard Schwartzkopf. This boiler was two-flued, the coal-dust apparatus being fixed in front of one flue, the other was divided longitudinally by a partition, so that the hot gases traversed the length of the boiler three times before passing into the chimney. It rested on three cast-iron stools, had no special setting, and, therefore, the loss of heat by conduction and radiation was unusually high. The results of the experiments did not, therefore, give a sufficiently clear measure of the effectiveness of the fuel-supply apparatus. Three kinds of coal-dust were used, of calorific values 7,323, 7,861, and 4,960 heat-units respectively. The heats utilised were 60·23 per cent., 59·10 per cent., and 59·66 per cent. respectively. The temperatures of the chimney gases and the percentages of carbonic acid were observed at intervals. The average temperatures during the three trials were 530° C., 522° C., and 478° C. respectively. The chimney losses were 19·92 per cent., 18·66 per cent., and 22·01 per cent. respectively of the total heat of the fuel; the radiation losses were 19·88 per cent., 22·25 per cent., and 18·33 per cent. respectively.

Remembering that with coal-dust fuel no dependence is made on the skill of the stoker, the above results appear to be better than is apparent at first sight.

The Author referred to trials made with the Schwartzkopf and Friedelberg systems, which he said were very satisfactory; he was not, however, in a position to give figures. He then discussed the advantages and disadvantages of coal-dust fuel. The advantages are—most perfect utilization of the fuel, complete smokelessness, small amount of manual labour required, favourable results from any kind of fuel that can be applied in the form of dust, adaptability of the system to any kind of requirements, preservation of the boiler, and rapid removal of the fire in case of danger. The disadvantages are—the grinding of coal into dust before it can be applied, the necessity of mechanical power, the deposit of ashes in the flues and tubes, and the dusty condition of the boiler-house, which is caused in some cases.

A. S.

Advantages and Disadvantages of Superheated Steam.

By — Walther-Meunier.

(Mittheilungen aus der Praxis des Dampfkessel- und Dampfmaschinen-Betriebes, 1895, p. 360.)

The Author first discusses the type of boiler and its influence on the question of the economy to be gained by the use of superheated steam. He points out that in Alsace-Lorraine, where superheating is practised extensively, the type of boiler in common

use is such that a considerable quantity of water is carried over by the steam from the boiler, and consequently economy may be effected by superheating. With a boiler producing dry steam, the margin for improvement by superheating will be much smaller.

Discussing the type of engine used, the Author says superheating will be advantageous in an engine having great clearance spaces, or in an engine with small clearance spaces but with a high rate of expansion. In the former case, the weight of superheated steam necessary to fill up the clearance space is less than that of an equal volume of wet steam; while in the engine with high expansion, the condenser action of the cylinder walls will be less with superheated than with wet steam.

The various types of superheaters, precautions to be taken with the same, and loss of heat in the steam-piping are discussed in turn.

It is sometimes supposed that, on account of its higher temperature, superheated steam will be more efficient for heating purposes than saturated steam; but the Author exposes the fallacy of this assumption, and shows that for heating purposes superheated can only be more advantageous than saturated steam when used to convert a mixture of steam and condensed water into dry saturated steam, but not when used to superheat the latter.

A. S.

The Kudlicz Fire-Grate for Steam-Boilers. By C. Cario.

(Mittheilungen aus der Praxis des Dampfkessel- und Dampfmaschinen-Betriebes, 1895, p. 389.)

In the Kudlicz system of firing, the draught is produced by steam-jets. The grate consists of cast-iron plates with conical openings for air. The draught of air produced is greater than can be obtained with a chimney of the usual height, and makes it possible to burn small coal. In order to test the value of the system, trials were made on two boilers of exactly the same dimensions, one of which was furnished with the Kudlicz grate. The trials took place on the 10th and 11th June, 1895. Feed-water was measured, fuel weighed, waste gases analysed, and all ordinary precautions taken during the trials. After the trials, the steam used for the blast was estimated by blowing into a condenser and weighing the amount of condensed water. With the Kudlicz grate, the fuel used was Davidson's West Hartley steam small coal; in the ordinary grate, lump coal from Oberschleswig was used.

The objects of the Kudlicz system—to use cheap coal and simultaneously produce more steam—were attained, the steam produced being 12·4 per cent. cheaper and 13·3 per cent. greater in quantity. This result may not, however, be attained in all

cases. In the case under notice, the chimney draught was too weak, so that the lump coal was not burnt to the best advantage. The thermal efficiency of the Kudlicz system was very low, 56 per cent., that of the ordinary system being 60·7 per cent. In a boiler with sufficient chimney draught, it is possible that, in spite of lower price of fuel, no advantage could be obtained by the use of the Kudlicz grate.

A. S.

Bechem and Post's System of Water-Spray Firing.

By Adolf Bechem.

(Gesundheits-Ingenieur, July 31, 1895, p. 227.)

A water-spray ventilator, manufactured by Mr. F. Kluge of Barmen, having been adapted to a soldering stove, it was observed that the fire became so fierce that a soldering-tool placed in it speedily became partially melted. The draught in the case of a smith's forge provided with this spray-ventilator produced a much greater heating effect than had previously been obtained by means of a Root's blower with a manometric pressure equal to 15·7 inches of water; the equivalent draught with the water-spray ventilator being only 0·12 inch. The Author was led, on this account, to attribute the rise in temperature to the decomposition of the minute globules of water and the combustion of the resultant gases. He accordingly purchased Mr. Kluge's patent for firing purposes. Attention is directed to the various ways in which water can be employed as fuel and its use in the production of so-called water-gas. When utilized as steam in the form of the steam-jet, water is much less effective as fuel in consequence of the fact that one molecule of water occupies the space of 1,700 molecules of steam and thus the chemical energy of steam is only the $\frac{1}{1700}$ part of that of water. The peculiar value of the water-spray as fuel lies in the fact that the finely divided drops of water come into far more intimate contact with the carbon on the hearth than the more elastic particles of steam, and moreover, as air is needed to support combustion, and as steam is at even temperatures specifically much lighter than air, there is, in the case of the steam-jet, a tendency on the part of the steam to separate itself from the atmospheric air, to rush forward to the fuel, and to prevent the free access of air to the carbon. Experiments with various kinds of fuel—coal, coke, &c.—in a smith's forge, have demonstrated that the water-spray is capable of producing the highest possible temperatures; the special arrangements of the jet for this purpose are explained. The use of the water-spray under fire-bars is extremely beneficial, as they are thereby kept cool and become rapidly protected by a coating of oxide, which serves as a preservative; there is, moreover, scarcely any perceptible waste in the bars. Experiments have also been

made with the use of the water-spray on a large scale for blast furnaces and other manufacturing purposes, and it is also capable of being used under certain conditions in domestic stoves.

The air-pressure needed to produce the spray is very moderate, even that due to water at a height of 33 feet suffices. The amount of water which can be consumed in the spray-form varies of course with the size of the fire-place from 2 to 6 gallons per hour.

The advantages of this system of firing are thus summed up by the Author. Great economy of fuel, absolute freedom from smoke, immediate production of extreme temperatures up to the melting point of platinum, adaptability of every description of fuel for the purpose, applicability of the system to all kinds of firing needed for industrial and domestic operations, and lastly the possibility of replacing by this means all existing regenerative systems and all plant adapted for firing with powdered coke, coal, &c.

G. R. R.

Locomotive Fire-Kindlers.

(Proceedings of the American Railway Master Mechanics' Association, 1895, p. 21.)

A mass of information is here published relating to the methods of kindling fires in locomotive engines, on a number of trunk line railways. The old method of lighting up with wood kindling, either with or without a blower, seems to be now superseded on several railways by the use of oil, either in the form of a liquid or of saturated briquettes. Oil has been found to raise steam rather quicker than wood, if both act under the same conditions of blower. In the matter of cost, oil seems to be much more suitable than wood, the relative total cost of raising steam being as one is to twelve on railways where wood is scarce. It has been found, generally, that lighting fires from the top of the fuel is more expeditious than from below the grate-bars; and an exceedingly important point mentioned by several speakers in the discussion, is that the oil-kindlers ignite the mass of coal more rapidly than wood, thus producing a box full of glowing coal ready for service sooner than wood kindling. The fuel oil and the saturated briquettes are of course highly inflammable, and must be kept in a safe place.

E. W.

Best Material and Specification for Iron Locomotive-Boiler Tubes.

(Report and Discussion, American Railway Master Mechanics' Association, 1895, p. 99.)

Locomotive boiler-tubes in North America are made almost without exception of iron or steel. Their cost varies from about one-half down to one-quarter of the value of tubes made of best brass mixtures in England. Considerable trouble is experienced in

maintaining these iron and steel tubes, partly due to defective welds in the "safe ends," and partly due to splitting of the ends; but the commonest cause of failure is corrosion in the form of "pitting." The general opinion among the master mechanics is, that steel tubes pit more readily than iron, and that, with bad water, there is more difficulty in keeping them tight, the ends becoming hardened from frequent rolling and caulking. Instances are given of steel tubes being thus destroyed with four months' wear. By means of corroding with acid, a polished section of different qualities of tubes and making an impression of the corroded sections with printers' ink upon paper, it is shown that some tubes are made of homogeneous steel, others of fibrous iron. Others are laminated, consisting of a layer of good iron outside and of common iron inside; while others again are made of iron upon both outer and inner surfaces, with a steel centre. Some members stated that, when using very bad water, corrosion takes place about equally in all classes of iron tubes, and they recommended the use of a common charcoal iron where the tubes require frequent renewal. The general opinion of the master mechanics approved the physical tests of drifting and bending a strip of the material double in both directions. They also recommend that tubes and safe-ends be made of "knobbled hammered charcoal iron." This material is made from charcoal pig iron, which has undergone a second refining process of being boiled with charcoal. "When it has become sufficiently cooked or refined, the iron is run into a cast-iron trough, about 15 feet long by 20 inches wide and 4 or 5 inches deep. The cinder or slag then comes to the surface, and, when water is placed upon the hot metal, is loosened and may be scraped off. The iron in the trough is then broken into small pieces and placed in a sunken furnace with charcoal, and the cold blast applied." When the iron thickens in this "knobbling" furnace, it is rolled into large balls and thoroughly hammered. The blooms are rolled into "break downs" or slabs, and these are repiled and rolled into "skelp iron," from which the tubes are made.

E. W.

A Locomotive with a Brick-lined Firebox. By M. Docteur.

(Revue universelle des Mines et de la Métallurgie, 3rd series, vol. xxx. p. 241.)

In the Author's system of locomotive-boiler construction the ordinary water-cased firebox is replaced by an external furnace tube of ⌓-shaped section, made of wrought-iron, which is closely lined with firebrick on the roof and sides, and closed in front by the copper tube plate of the cylindrical body of the boiler. The heating surface and steam space lost by this arrangement are made good by increasing the diameter of the boiler shell, so as to allow of the use of a larger number of tubes, and the addition of an

external steam chest of a cylindrical form. This system has been tried upon a locomotive of a pattern now abandoned (No. 512) of the Belgian State Railways, which has been reconstructed with a cylindrical boiler 13 feet 1½ inch long and 5 feet in diameter, containing 306 tubes, and an external furnace 7·7 feet long and 6 feet broad inside, the total length of the boiler by these changes, together with an enlargement of the smoke-box, being increased about one-fourth. The firebrick lining is solid on the fire-door side, but that on the sides and roof is built up of hollow bricks, giving a series of rectangular passages through which air is drawn from the outside and discharged over the top of the layer of fuel on the grate, where it ensures complete combustion and prevents loss of heat by radiation, the temperature of the outside casing of the brickwork being lower than the outer plates of an ordinary firebox. The air-ways are arranged in seven parallel rings, which open alternately on opposite sides to the grate, giving a uniform distribution to the supply over the bed of fuel. According to the drawing which accompanies the original, each air-way extends from the bottom of the firebox on one side, with external openings, over the crowns, to the bottom on the other side, with an internal opening that discharges the heated air just above the fire.

The engine (of the Mogul type) has six coupled wheels of 4 feet 9 inches, and a two-wheeled bogie of 2 feet 9 inches diameter, two cylinders of 19 inches diameter and 26 inches stroke, and works at a pressure of 9½ atmospheres. With the original boiler the heating surface was 1,260 square feet divided as follows: 100 square feet in the firebox, and 1,160 square feet in the tubes. As reconstructed, the total surface is 1,723 square feet or 15 square feet in the firebox and 1,708 square feet in the tubes. It went to work on the 14th of February, 1894, being employed on the goods-train service between Luttre and Brussels, Antwerp, returning by the line of Braine le Comte, with results of a very satisfactory character. Using a mixture of 3 parts of dry to 1 of caking coal, the average evaporation has been 8 and the maximum 9·25 parts of water to 1 of fuel, while the average consumption has been reduced from 78 to 56 kilograms per mile for a total distance run of 11,579 miles. For the period February to April, 1894, the saving of fuel was 17·66 tons on the standard allowance of 59·9 tons, or 33 per cent.

During the progress of the experiments the heating surface was reduced at intervals by plugging rows of tubes in order to determine the relative value of firebox and tube surface. The numbers of the tubes stopped were successively 20, 39, 51, 67, 76 and 100. With the last of these reductions the steaming power of the boiler was impaired, and the necessary pressure could not be maintained, but when the number of ineffective tubes was 76, giving a total surface of 1,298 square feet, or about the same as that of the standard type of goods engine No. 25, the evaporation was sufficient to maintain the regular working of the

trains. The Author therefore concludes that there is no difference in evaporative power between a square foot of firebox surface and an equal area in the tubes. The circulation of air through the passages in the lining is however an essential to the working of the system, as it was found in a final experiment that when these were stopped so as to prevent the admission of heated air above the grate, there was an immediate fall in steam-production and an increase in coal-consumption owing to imperfect combustion.

The chief source of danger to the lining arises from sudden exposure to cold air when the bricks are at a white heat. It is therefore preferable to let the fire burn down gradually instead of emptying the grate at the end of the journey. On the other hand, steam may be raised very rapidly, not more than 1½ hour being required, even when the boiler is filled with cold water, while, for the ordinary locomotives in the same service, three hours is regarded as a minimum.

The saving in first cost by not using a copper firebox is estimated by the Author at £200 per engine of the class described.

The cost, materials and wages included, of renewing the lining, which can be done in three days by an ordinary bricklayer, is below £4.

H. B.

Leakage in Flame-Tube Boilers. By — Strupler.

(Mittheilungen aus der Praxis des Dampfkessel- und Dampfmaschinen-Betriebes, 1895, p. 296.)

The Swiss Union of Steam-Users made observations on the leakage of boilers which came under their notice during the course of ten years, from the 1st of August, 1884, to the 31st of July, 1893. Of one-flue boilers, 416 came under notice. Of this number fifty-nine, that is, 14·2 per cent. of the whole, became leaky at one time or other. In order to note the effect of length on the leakage, they were divided into four classes, under 5, 6, 7 and 8 metres respectively; the percentages of leakage being 7, 12·2, 21·4, 35·3 respectively. Classifying them according to the thickness of plate, under 11, 14, 17 and 21 millimetres, the percentages of leakages were 3·9, 15·7, 20·8 and 61·5 respectively.

One hundred and eighty-four two-flue boilers came under notice, of which seventy-eight, that is 42·4 per cent. of the whole, leaked. A similar classification is made according to length and plate thickness.

From these observations it seems that leakage increases with length of boiler and with plate thickness, so that the longer the boiler and the thicker its plates the more carefully it ought to be handled.

A. S.

Tubulous Boilers in the French Navy.[1]

By Assistant-Engineer JOHN K. ROBISON, U.S. Navy.

(Journal of the American Society of Naval Engineers, May, 1895, p. 365.)

The article contains the results of observations of the practical working of tubulous boilers in France with some data regarding the most important of them. In nearly every vessel under construction for the French Navy the boilers are of the tubulous type. For torpedo boats and other vessels of small size the boilers are generally of the Thornycroft or Normand type, and for larger vessels, either Belleville, d'Allest, or Niclausse. The Belleville is the oldest. The small quantity of water in this boiler makes the use of an automatic feed-regulator imperative. This regulator works well when in order, but fails often enough to destroy all confidence in it, and serious accidents often result from its failure, the water disappearing entirely from the boiler. The slow circulation causes rapid deterioration of the tubes if the water is not pure, especially in the lowest row. Priming frequently occurs, especially when the engines are started; and it has been estimated by engineers who have worked these boilers for several years that the amount of water in the steam at the cylinders is seldom less than 10 per cent. Air-pumps for forcing compressed air into the furnace to ensure the mixture of the gases, and a special design of feed pump, are considered as necessary adjuncts to the boiler, and cause an exaggerated amount of repairs as compared with those of an ordinary boiler. The repairs can, however, be executed in a shorter time than in the case of the cylindrical boiler; and this may be taken to counterbalance in a large degree the greater frequency of breakdown. The advantages of the Belleville over the Scotch boiler, most appreciated in France, are the great gain in the weight of the steam-producing plant, less forcing required, and greater ease of raising steam. The pressure that can be used is practically unlimited, and the parts are small and can be easily removed without cutting holes in decks. When salt water has been used in Belleville boilers the tubes were found to be eaten away and the rods of the feed-regulators rapidly became covered with incrustation which prevented them from acting and caused the destruction of the boilers.

D'Allest boilers are lighter than the Belleville type, but the floor space occupied is about the same for equal powers. Having fewer auxiliaries they require less repair, and as they contain more water they can be fed by hand. They are more economical, comparing well in this respect with Scotch boilers, while with the Belleville type the loss in coal has been, in some cases, as much as 42 per cent. Belleville tubes being free to expand, while those of

[1] This Paper is given *in extenso* in *Engineering*, November 8, 1895, p. 587.

the d'Allest are secured at both ends, the latter are more liable to leakage.

The Niclausse boiler is about the same weight per foot of surface as the d'Allest, and both stand forcing better than the Belleville. They also both give dry steam. Niclausse tubes being secured only at one end are very unlikely to give trouble from leakage. All repairs can be made from the front of the boiler, and a Niclausse tube can be mounted or dismounted in less time than in any other type of boiler. The d'Allest is the most economical of the three types, and is, in the opinion of French engineers, likely to be the boiler of the future. Particulars of the Belleville boilers, of the Messageries Maritime Steamer "Australien" are given, together with a very full description of the manner of working them. Particulars of the d'Allest boilers of the Carnot, and of their management, are also given, and a detailed description of the Niclausse type.

Three second-class cruisers, identical in all other respects, are being severally fitted with the three types of boiler under consideration, in order to obtain a fair comparison of the rival systems. These are the "Bugeaud," "Chasseloup-Laubat," and the "Friant." The designed I.HP. is 9,000. The Belleville boilers cost £24,140, the d'Allest £20,000, and the Niclausse £24,600. The weights of machinery compare as follows: With Belleville boilers, 801 tons; with d'Allest, 757 tons; with Niclausse, 760 tons.

S. W. B.

Comparison of Austrian and American Safety-Valves.

By — Radinger, — v. Pichler and — Zwianer.

(Mittheilungen aus der Praxis des Dampfkessel- und Dampfmaschinen-Betriebes, 1895, p. 341.)

Professor Joh. v. Radinger having brought a few "Pop" safety-valves from America on his return after the Chicago Exhibition, a Committee of the Austrian Union of Engineers and Architects was appointed to make a comparative trial.

The trial took place in the locomotive-house, West Station, Vienna. A locomotive was specially prepared for the experiments. A pair of safety-valves of the ordinary construction used on the State railways and a pair of Pop safety-valves were fitted. Sections of the two safety-valves are given. The diameter of the normal valve is 4·5 inches and that of the Pop safety-valve 3·7 inches. During the experiments the European and American valves were used alternately, the pair not in use being screwed down.

The boiler pressure was first raised to ½ atmosphere, and then by means of an auxiliary blast tube a powerful draught was obtained. The steam-pressure gradually rose, the safety-valves lifted until a condition of equilibrium was established between the heat carried off by the escaping steam and that supplied by the

furnace. During the trial no feed-water was supplied to the boiler.

The Austrian normal safety-valve showed an excess of pressure of 2½ atmospheres above the loaded pressure, the Pop safety-valve an excess of about ¾ atmosphere. The rise in pressure occupied seven minutes in the case of the Austrian safety-valve, and thirteen minutes in the case of the Pop valve. These results are discussed, and the conclusion is drawn that the Pop valve is nine times as effective as the Austrian.

The Committee therefore strongly recommend the introduction of the Pop safety-valve on account of the greater security it offers.

A. S.

The Wear of Driving-Wheel Tires.

(Annual Report of the American Railway Master Mechanics' Association, June, 1895, p. 228.)

This report contains a minute investigation into the amount of wear, and the forces causing that wear, in a number of different classes of locomotive engines running upon several of the trunk-lines west of Chicago. It includes a mathematical investigation into the actual pressure of each driving-wheel upon the rail during an entire revolution at different speeds, taking account not only of the weight upon the springs and dead weight of the wheels and axle, but also the centrifugal force of the overbalance in the drivers, the effect of the acceleration and retardation of the reciprocating parts, and the angularity of the main rod. A mathematical investigation is also worked out to ascertain the total rotative force at the rail during an entire revolution, and for the same speeds as those for which the pressures were calculated. These results are tabulated, and numerous diagrams are drawn, showing the effect of each of these laws upon the ordinary conditions of working these particular locomotives, *e.g.*, it is shown that the total pressure upon the rail under all the driving-wheels of one locomotive running at 60 miles per hour varies during each revolution between 26·8 and 49·5 tons. The effect of each principal disturbing force is noted upon diagrams representing the average amounts and irregularities in tire wear of fifty-three ten-wheeled goods-engines with 19 inches by 26 inches cylinders. The greatest amount of irregular wear is ascribed to slipping, and a particularly bad place in the left main driving-tire is said to be due to the slight slip caused when the main rod passes the forward centre, and suddenly thrusts the adjacent wheel backwards through a distance equal to the "slack" or lost motion in the bearing-shoe and wedge. This action is immediately followed by the more serious conditions of the period of maximum coefficient of slip, extending over about 20° of the circumference, and corresponding with a flat place worn upon the tire.

One general conclusion reached during the discussion is that high speed has a greater effect than heavily loaded wheels upon the wear of tires. Another conclusion is that the maximum wear is not caused by the increase of pressure due to overbalancing, though this is shown to have a disturbing effect, but it is due most particularly to the slipping of the wheels, and most largely to that slipping which occurs when the engine is just starting. The Report concludes with the following recommendations:—

1. Driving-wheels should have ample weight for adhesion.
2. Connecting-rods should be as long as is consistent, in order to decrease the effect of angularity.
3. The weight of the reciprocating parts, and consequently the overbalance in the driving-wheels, should be as light as possible.
4. As small a proportion of the reciprocating parts should be balanced as is consistent with smooth-working machinery, and good riding conditions.
5. The driving-axle boxes, shoes, and wedges should be well maintained, and kept properly adjusted.
6. Have a careful and competent driver in charge, who will avoid slipping the driving-wheels.

E. W.

The Shaft of the Laval Steam-Turbine. By A. Foppl.

(Der Civilingenieur, 1895, p. 333.)

In a shaft making several thousand revolutions per minute it is impossible to centre the weight so accurately as to get completely rid of centrifugal force. In order to avoid vibration it has been customary in such cases to support the shaft so that the centre of rotation is free to pass through the mass-centre of the rotating body.

In the Laval steam-turbine, which in some cases makes 30,000 revolutions per minute, it has been customary to make the shaft so flexible as to offer very little resistance to bending, so that it may be possible for the principal axis of the rotating mass to coincide with the axis of rotation.

At first sight it seems that if the shaft be slightly bent and then rotated, the unbalanced centrifugal force will tend to increase the bending. Mr. Klein, assistant of the Technical High School, Munich, made some experiments on the subject, and found that when a flexible shaft is set in rapid rotation it takes up automatically such a position that the mass-centre lies on the axis of rotation, and shock from centrifugal force is thus eliminated. He also found that to obtain this effect the speed of rotation must have a certain minimum value; if this critical speed of rotation is ex-

ceeded the rotation goes on quite smoothly, if the speed of rotation be less an intense vibration is set up.

The Author gives an explanation of this phenomenon in simple non-mathematical language, which can be understood by anyone having a sound knowledge of elementary mechanics. He then investigates the subject mathematically, and finds that the critical speed of rotation is given by the formula—

$$N = 300 \sqrt{\frac{P}{Q}},$$

where N is the number of revolutions per minute, Q the weight of the rotating body in kilograms, P the force in kilograms required to produce a deflection of one centimetre of the shaft. At N revolutions the vibration of the shaft is greatest, if the speed is increased beyond N a steadier motion is obtained. From the experiments it seems that for very steady running a speed of $1\frac{1}{2}$ N–2 N is necessary.

A. S.

Test of a 100-*HP. Laval Steam-Turbine.* By C. Compère.

(Resumé de la Société des Ingénieurs civils, Paris, 1895, p. 248.)

In 1894 the jury of the section of Mechanical Industries at the Bordeaux Exhibition requested the Author to test the consumption of a Laval steam-turbine of 100 HP. Arrangements were made to measure the steam fed from a separate boiler, and two tests were carried out, one at the normal power of 98 HP., the other at half-load, 49 HP. The consumption of steam was, working condensing:

At full load: 32·23 lbs. per kilowatt, 23·71 lbs. per electric HP., 20·15 lbs. per brake HP.
At half-load: 40·30 lbs. per kilowatt, 29·65 lbs. per electric HP., 23·80 lbs. per brake HP.

The efficiency of the dynamo used was taken at 85 per cent. at full- and 80 per cent. at half-load.

Mr. Compère also made a supplementary trial in the workshops of Messrs. Bréguet on a 74-HP. Laval steam-turbine for electric-lighting. As a separate boiler could not be spared, the steam-consumption was measured by means of a surface-condenser, and in a second trial, with a condenser giving a better vacuum. The object was to show, that whatever the vacuum, the consumption, without reference to the work done, remains the same. The first trial lasted three hours. The vacuum was 21·6 inches for the first two hours, and 17·7 inches during the third; the steam-consumption did not vary. In the second trial the vacuum was 25 inches, and the consumption of steam 32·47 lbs. per kilowatt.

These results prove that the quantity of steam used depends

only on the steam-pressure, and the section of the pipe, and not on the exhaust pressure. If a steam-turbine is meant to work economically with a small load, it is necessary to adjust the section of the orifice, and not the pressure of the steam by throttling. This is confirmed by an experiment made on a 10-HP. Laval steam-turbine, without condensation, by Mr. Vinçotte, Manager of the Belgian Steam-Boiler Association. The consumption was 49·2 lbs. at 9·7 HP., and rose to 59·4 lbs. at 6·5 HP., and to 89 lbs. at 3·31 HP.; by throttling the admission valve the pressure of the steam was very much reduced on entering the turbine.

B. D.

The Pelton Water-Wheel.

The Report of the Committee of the Franklin Institute.

(Journal of the Franklin Institute, September, 1895, p. 161.)

The Pelton water-wheel is a reaction wheel, its power being derived from the impulse due to the discharge of water under pressure upon it, through a small nozzle, of a size varying with the available head of water and the power required.

The wheel works in a vertical plane turning upon a horizontal axis, and its bearings are fixed upon a wooden or metal frame, to which the nozzle is attached.

Over the wheel, but not touching it, is placed a cover for directing the discharged water to the tail-race.

Its efficiency is almost independent of its speed, because the size of the buckets is arranged to suit the diameter of the jets of water. The shape of the buckets is two cups joined together side by side, the junction being pointed and facing the jet of water so as to divide it into two parts, sending one to the right and one to the left, the direction of the jet being nearly reversed on leaving the bucket and its velocity nil. For maximum efficiency the velocity of the buckets is one-half that of the jets. Under these circumstances an efficiency of 80 per cent. is common. The centrifugal friction governor has been used to a very great extent to act upon a double-geared bevel friction-wheel which opens and closes a butterfly valve. The wheels are made in sizes from 4 inches diameter, weighing 20 lbs. complete, for driving sewing machines, &c., to wheels of 10 feet or 20 feet in diameter, although the larger sizes are not for the purpose of increasing the power, but of reducing the angular velocity so as to make direct connection to the shafts of the machinery to be driven.

From 3,000 to 5,000 HP. can be obtained by applying from three to five jets under a head of 150 feet to a wheel from 10 feet to 15 feet diameter.

At the Comstock mines, Virginia City, Nevada, there is a 36-inch Pelton wheel, made of a solid steel disk with phosphor-

bronze buckets riveted to the rim. The water for the jet has a head of 2,100 feet, and the wheel runs at the rate of 1,150 revolutions per minute, and with ½ inch diameter nozzle develops 100 HP., using 32 cubic feet of water per minute. The weight of the wheel is 200 lbs.

A. W. B.

Report upon the "Magenta."

By Vice-Admiral VALLON, President of the Special Naval Commission.

(La Marine Française, 1895, p. 738.)

The original design for the "Magenta," prepared by Mr. C. Huin, Naval Constructor in 1880, was as follows:—

Length	316 feet.
Beam	64 „ 6 inches.
Mean draught	26 „ 3 „
Displacement	9,706 tons.
I.HP.	6,871
Speed	14·5 knots.

She was to have 13·4-inch guns mounted in barbettes on the middle line. The design was altered in 1881 to enable four guns of 13·4 inches to be carried, the centre gun of the original design being replaced by two mounted in barbettes on each side. To allow of this increase in weight of armament, the length and beam were slightly augmented, and the displacement increased by 716 tons.

Numerous other modifications were introduced during construction, among others the machinery was increased in power in order to attain a speed of 16·2 knots instead of 14·5. These alterations involved an increase of weight of 247 tons, resulting in an increased immersion of about 6 inches. The effect upon the stability was to reduce by nearly one-third the length of the righting-arm. Instead of a righting-arm of 3·11 feet, which the vessel would have had as first designed, she has one of 2·3 feet as completed, and this is reduced to 1·75 feet as coal and stores are consumed. In the intact condition the maximum righting moment occurs at an angle of 35° 48′, and stability vanishes at 56° 30′. At an angle of 4° 40′ the top of the armour belt is submerged, and at an angle of 8° 45′ the lower edge of the belt on the opposite side is level with the water-line. With three of her large guns trained on the broadside the vessel heels 5° and puts her belt under water, and if, with her guns in this position she turns a circle, the initial heel is 14°, and a steady heel of 13° is maintained while turning. This causes the water to leave the tops of the furnaces dry in places, and there is danger of explosion. It was estimated, but not tried on account of the risk of injury to the boilers, that turning at full speed and under full helm with the guns trained

on the broadside, the initial heel would be 16° and the steady heel 15°. The guns can be manœuvred easily with the ship inclined 14°, but the limits of elevation and depression are such that they could not be fired on one side except at angles below the horizon, and on the other that they could not be sufficiently depressed. She could always keep her guns trained upon a ship round which she was circling, as the effect of the weight of the guns is then to counteract the heel due to centrifugal force, and with three guns trained towards the centre of her circle her maximum inclination would not exceed 6°, and the lower edge of the belt turned towards the enemy would be still immersed. As the guns are worked by steam-power, if she is surprised with her fires out, or if her boilers are injured the heavy guns cannot be worked at all. The committee recommend that guns which can be worked by hand or by machinery, independent of the motive power of the vessel, should in future be preferred.

At full power under forced draught the coal consumption is $2\frac{3}{4}$ lbs. per I.HP. per hour, and at natural draught about 2 lbs. The exact speed which was estimated, viz., 16·2 knots, was obtained on trial, but it is not expected that more than 15 knots with forced draught will be obtainable at sea; and the committee report that they consider this is not sufficient, in view of the speeds obtained with similar vessels in foreign navies. They also find that the coal-capacity is very inadequate, being 610 tons only. Comparing her with the "Anson," one of the British "Admiral" class, of the same displacement and nearly the same dimensions, they point out that the "Anson" has 734 more horse-power, 1·2 knot higher speed, a thicker armour belt, an armoured deck of the same thickness, within $\frac{1}{8}$ inch, 206 less men, and double the quantity of coal. At full speed the bow is depressed about 2 feet, and the vessel has a tendency to be unsteady on her course, and a source of danger to her consorts when steaming in close order.

The "Magenta" was eight years and six months on the stocks, and it was thirteen years from the time when the original plans were approved before the vessel was ready for sea. She cost £803,618.

The condition of the vessel as regards stability when she is no longer intact forms the subject of a separate report. The committee find that if the vessel be injured by ram or torpedo below the belt, she becomes *hors de combat* if not lost. The inclination due to the admission of water to engine- or boiler-rooms is so great that the fires must be extinguished to avoid explosion, and although the vessel may float in smooth water her condition is precarious and her guns cannot be fought. If she is badly injured in her unarmoured parts above the belt, so that water is admitted freely upon the armoured deck, an inclination of $6\frac{1}{2}$° is sufficient to cause her to capsize and sink. The conclusions arrived at are, that although the stability is sufficient with the hull intact, it may be compromised by injury; that the speed should be 17 knots or 18 knots at sea with natural draught; that the coal-supply is

insufficient; that the multiplication of machinery, the vessel having 109 auxiliary engines, is injudicious; that the guns are of too many different calibres; that the boilers (marine type) are unsatisfactory; and that the watertight subdivision is insufficient. Nevertheless, the opinion of her commander is said to be that she is a superb ship, not perfect but open to criticism on many points and capable of improvement, but on the whole a very fine and very powerful vessel.

S. W. B.

The American-Line Steamer "St. Louis."

(Engineering News, August 15, 1895, p. 99.)

This vessel and her engines were designed and constructed by the William Cramp and Sons Ship and Engine Company of Philadelphia for the new American line running between New York and Southampton. The ship was commenced July, 1893, and completed in May, 1895. There is a double bottom, a space of 4 feet 6 inches separating the inner and outer bottoms. The frames and plating at the stern are swelled out to enclose the screw-shafts, forming two almost horizontal fins, there being no exposed section of shaft and no shaft brackets. The shell plating is carried up to the promenade deck, forward and aft, forming a forecastle and poop.

There are five decks, exclusive of a shade or boat deck. Above the boat deck is a superstructure containing the officers' quarters and the chart-house, and above this again are the bridge and wheel-house. There are seventeen main water-tight compartments—all the doors in bulkheads, except those in the longitudinal bulkhead between the engine-rooms, are above the load water line.

The twin screws are driven by two quadruple expansion engines together developing 20,000 I.HP. Each engine has six cylinders and four cranks. The high-pressure cylinders are placed over the low-pressure cylinders. There are two high-pressure cylinders 28½ inches diameter, one first intermediate cylinder 55 inches diameter, one second intermediate cylinder 77 inches diameter, and two 77-inch low-pressure cylinders. The stroke is 5 feet. Steam-jackets are fitted to the second intermediate and low-pressure cylinders. The framing is of cast steel. All the cylinders have balanced piston-valves, the low-pressure cylinders have two each. There are ten boilers of marine type with sixty-four furnaces in all. The total grate area is 1,144 square feet, and the total heating surface 40,320. The working pressure is 200 lbs. The Howden system of forced draught is used, air being supplied by eight 80-inch Sturtevant fans. The bunkers contain 2,500 tons, or sufficient for eight days' steaming, the consumption per day being 300 tons. Worthington feed-heaters and feed-pumps are fitted. Each con-

denser has four independent Worthington air-pumps, and the total cooling-surface is 26,170 square feet.

The vessel accommodates 350 first-class, 200 second-class, and 800 third-class passengers, making a total, including a crew of 400, of 1,750 persons. 1,820 tons of freight can be carried in the holds. Provision is made for converting the ship into a commerce destroyer, carrying light guns.

She is to obtain a sea-speed of 20 knots on a four hours' trial. The following are the principal dimensions—

Length over all	554 feet.
„ between perpendiculars	536 „
Beam	63 „
Depth	42 „
Draft	26 „
Displacement	10,000 tons.

S. W. B.

New American Revenue Cutters.

By First Assistant-Engineer CHARLES A. MCALLISTER, U.S.R.C.S.

(Journal of the American Society of Naval Engineers, August, 1895, p. 361.)

Two first-class revenue cutters—one intended for service on the great lakes, and to be stationed at Milwaukee, and the other for service on the New England coast, to be stationed at Boston—are being constructed by American firms. The Milwaukee cutter, building at the Globe Iron Works, Cleveland, Ohio, will cost £29,740, while that for Boston, building at the Atlantic Works, East Boston, will cost £32,000.

The general dimensions of both are—length over all, 205 feet; length B.P., 188 feet; breadth moulded, 32 feet; depth, 17 feet. The Milwaukee cutter will have a displacement of 906 tons on a mean draught 10 feet 10 inches, and the Boston cutter will displace 980 tons at a mean draught of 12 feet 3 inches. The Boston cutter is of composite build, the other will be all steel. Particulars of the scantlings are given.

The vessels are armed with one 6-pounder rapid-fire gun on the forecastle deck and two 1-pounders on the rail aft. They also carry a bow torpedo tube to enable them to act as auxiliaries to the navy in time of war. Four boats are carried, one of them a steam launch. The cruising speed is expected to be 16 knots, and it is thought they will be capable of attaining 17½ knots for a short run. The total I.HP. will be 2,000, the engines running at 160 revolutions per minute. They are of the triple-expansion type, having cylinders 25 inches, 37½ inches, and 56¼ inches diameter respectively, and a stroke of 30 inches. The condenser cooling-surface is 3,000 square feet. Independent single-acting air-pumps are employed. A search-light is carried, and the internal illumina-

tion is electric. Steam is supplied by four single-ended marine boilers, having a total heating-space of 5,200 square feet, and a total grate-surface of 168 square feet. Forced draught on the closed-stokehold system is employed.

S. W. B.

Water-Motors as Marine Dynamo Drivers.

By Lieut. F. J. Haeseler, U.S. Navy.

(Proceedings of the United States Naval Institute, 1895, p. 281.)

The feasibility of the use of water-motors on board ship for the purpose of driving the dynamos and also the ventilating fans is presented for consideration. An outline of the proposed scheme is as follows: That there should be duplicate dynamos of a high-speed type, coupled direct to the shafts of water-motors actuated by water from the steam fire-pumps, also in duplicate; the waste water to be pumped overboard by a pump in the dynamo-room, or pumped into flushing system with an overflow, or to be returned through a return pipe to the pump in the fire-room.

In connection with the above outline the points to be considered are: (1) original cost; (2) expense of maintenance; (3) advantages and disadvantages.

The comparative cost of the usual steam installation and the proposed water-motor is given as £1,300 and £276 respectively, showing a saving in favour of the latter of £1,024. A saving in the cost of maintenance is estimated under the head of attendance at about £9 per month, while it is believed that great economy can be effected in repairs by substituting for the high-speed engine a machine that is practically indestructible in use and never needs repairs. It is estimated that the expenditure in lubricants would be reduced to one-half the present figure. Among the many advantages claimed for the water-motor system, reduced weight, space occupied, and heat in dynamo-room are the most important.

The water-motor is also recommended for driving the ventilating fans. To avoid air-ducts running through the ship and piercing water-tight bulkheads, it is proposed to have a separate blower in each main water-tight compartment driven by motors. It is claimed that a saving would result in coal, cost, weight and maintenance, besides ensuring a more effective system of ventilation.

The efficiency of the steam system at full load is estimated at 80 per cent. and that of the water system at 71·2 per cent. At half load the steam system should have an efficiency of 67 per cent. and the water system of 65½ per cent. Against the superior efficiency of the steam plant must be set off the waste due to an electric motor and blower required to keep the room cool, adjuncts which are not required with the rival system.

S. W. B.

Annealing Armour Plates by Electricity.[1]

By W. W. HANSCOM, Chief Engineer of the Union Ironworks, San Francisco.

(The Iron Age, New York, August 29, 1895.)

The nickel-steel armour plates, as furnished the later vessels of the United States navy, are, by the Harvey process, hardened on the face to a depth varying between ½ inch and ¾ inch. This face is such that it successfully resists the hardest steel drill that can be made, and, as it is required to drill and tap numerous holes in the plate in its final position, it was necessary during the hardening process to protect the desired places against preventing contact with the carbonizing material. The operation was not entirely successful, however, as it was found upon trial that, although a number of the places were sufficiently soft to be worked, others immediately adjacent were as hard as the unprotected portions. A number of attempts were made to locally anneal these hard spots by means of the oxy-hydrogen blow-pipe and other apparatus, the most successful being that offered by the Thomson Electric Welding Company of Lynn, Mass. It was found impossible by other means than electricity to apply sufficient heat in a concentrated form to attain the desired results, as the large mass of metal surrounding the spots conveyed the heat from them as fast as it was supplied.

An equipment for annealing the armour plates of the U.S. battle-ship "Oregon" has recently been installed at the Union Iron Works by the Thomson Electric Welding Company. The apparatus consists of an engine driving an alternator with its exciter, a regulating rheostat, and a transformer-annealer. The engine develops, at 450 revolutions per minute, 55 HP. The alternator, of 40 kilowatts capacity, has six pole-pieces with their coils coupled in multiple-series, two sets of three in parallel being joined in series. The armature is of the toothed type, also with six coils, three pairs of coils in parallel being joined in series. It is wound for an output of 135 amperes at 300 volts, when making 1,000 revolutions per minute. A pulley on the end of the armature-shaft drives the exciter, a D-type shunt-wound generator of 110 volts, at 2,000 revolutions per minute. Its terminals are connected to the alternator field-magnet coils through the regulating rheostat, a cylindrical frame having german-silver coils thrown into or out of the circuit by a contact-arm. The coils are protected from mechanical injury by the wire-gauze covering, which permits of a constant circulation of air. The transformer-annealer is of the shell type, and consists of an outer core of laminated iron surrounding both primary and secondary coils, the former being wound on a form and enclosed within the latter, which is a hollow copper casting made in halves to receive it, and then

[1] An illustrated account of this process will be found in the *Electrician*, November 22, 1895, p. 119.

bolted together. The remaining space is filled with oil for insulation and for conducting away the heat generated in the primary coil. The secondary coil has but a single U-shaped turn, to the ends of which are bolted variously shaped copper contact-pieces which are hollow to allow water circulating within them, thus preventing the heat of annealing from reaching the coils. The yoke from which the transformer is suspended by two trunnions, as well as the contact-pieces, permit of the transformer being swung at any desired angle and brought against any part of plates already in position.

In the operation of annealing, the contact-pieces are brought up against the brightened surface of the plate and wedged into position, straddling the spot to be annealed, after the regular rheostat has been adjusted to a point reducing the primary current to a minimum. The distance between the contact-pieces for a hole $\frac{7}{8}$ inch in diameter is $1\frac{3}{4}$ inch. When contact is established between the contact-pieces and the plate, a slight humming noise notifies the operator, and the primary current is gradually increased to its maximum. A bright red spot then appears under each contact-piece. The intense local heat at these spots causes the plate to expand outwardly in the direction of least resistance, forming slight mounds, from which circles of gradually changing colour slowly approach the centre. The primary current is maintained till the plate has become sufficiently heated to char or even ignite a pine stick held against it, and is then gradually decreased until it has again reached the minimum. The first or heating period requires about three minutes, during which the secondary current has increased from 3,500 amperes to 6,000 amperes at 4 volts. The second or cooling period requires between ten and twelve minutes, in order to prevent the sudden chilling of the spot due to the surrounding mass of metal, and to ensure a perfect anneal. The plate at the spot of annealing presents a dark blue colour, elliptical in shape, with a major axis of 4 inches and a minor axis of $2\frac{1}{2}$ inches, and is readily drilled and tapped.

T. A. B. C.

An Electric Ferry-Boat Service at Bergen. By J. Trumpy.

(Elektrotechnische Zeitschrift, 1895, p. 240.)

The position of the town of Bergen is somewhat peculiar, in so far as the harbour—a long and narrow inlet of the sea—practically separates it into two halves: means of communication between the opposite parts of the town have therefore always been necessary, and of late years increasingly so. The conditions of working were naturally favourable to the clean, commodious, and rapid service, which is afforded by electric launches on sheltered waters near an economical source of power: and hence the adoption of such a system, which has now been in successful and appreciated operation

for about a year. Four lines of ferry are worked by the electric boats; the distances covered are respectively 306, 340, 660 and 682 yards; eight boats in all are employed. Each measures 26 feet in length by 6 feet 6 inches wide, with a draught of 2 feet 8 inches. The passenger capacity is eighteen persons, and the displacement 6 tons. The stern and stem are shaped alike, somewhat bluntly; and each end is provided with distinct screw and rudder. Both screws are, however, attached to the same shaft, which runs the full length of the boat, and the electric motor is mounted directly upon it at the centre. The motors are all series-wound, and weigh each about 6 cwts., giving 3 HP. As far as possible, they are rendered waterproof, so that accidental leakage into the boats may not destroy their power of working. The accumulators employed for driving the motors are placed partly under the side seats, and partly under the foot-boards; their total weight for each boat is a little over 2 tons, there being thirty-two cells (coupled in series), each weighing complete about 150 lbs. The average speed obtained on a consumption of energy equal to 2,300 watts is about 7 feet 6 inches per second.

A five-minute service from 7 A.M. to 9.30 P.M. is maintained in the winter, with, of course, a correspondingly extended one in the summer. The daily run of each boat is from 38 to 40 miles, and the total number of passengers carried about 1,800 persons. For recharging the accumulators there is a small power-house provided, which contains a boiler of locomotive type, with compound engine driving a 30-HP. dynamo generator.

The outlay amounted to £2,500 for the eight boats with spare parts, and £2,000 for the charging station, boat-house, and landing-stages, &c., or a total sum of £4,500.

F. B. L.

The Transformation of Carbon into Plumbago. By J. Reyval.

(L'Éclairage Électrique, vol. iv., 1895, p. 454. 6 woodcuts.)

The Author states that numerous experiments, and specially those made by Mr. Moissan,[1] have proved that plumbago is a stable variety of carbon at high temperatures. All that is required is a sufficiently high temperature, such as can be easily obtained by aid of the electric furnace. It was found that the operation of converting the outer layer of a carbon block into plumbago was of service commercially, as by this means the conductivity and the power of resistance to chemical reactions were increased. In order to obtain commercial results it was necessary that the cost of the operation should not raise the price of carbon so treated beyond the increase in their value. Messrs. Girard and Street have been working on this problem, and have devised several special kinds of electric furnaces for the purpose of transforming carbon into

[1] Comptes Rendus de l'Académie des Sciences, Paris, vols. cxix., cxx.

graphite. The Author proceeds to describe these furnaces and illustrates sections of them. The broad principle is as follows: The carbon rod to be converted into graphite is passed through a block of refractory material, having a small heating chamber in the centre.

A second carbon enters this chamber at right angles to the first carbon, and an arc is maintained between the two. The speed at which the first carbon is caused to pass through the chamber is regulated in accordance with the temperature to which it is to be raised. In the case of circular carbons for arc lighting the carbon rod to be converted is given a motion of rotation as well as of translation. The effect is then as follows: the action of the arc leaves a spiral trace on the carbon to be converted. The size of this trace on a 0·55-inch diameter carbon with a 40-ampere arc, was found to be $\frac{1}{16}$ inch broad and about $\frac{1}{16}$ inch in depth, and this represents the carbon converted into graphite.

It was found that an increase in the temperature in the chamber increased both the breadth and depth of the trace. By regulating the speed with the temperature the whole surface of the carbon can easily be converted. In this process the carbon to be treated was connected to the positive pole. After describing other details of the process and special cases in which alternating currents are used, the Author gives the following particulars of the alterations effected:—The electrical conductivity of the carbons is increased in the ratio of 1 to 4, and the conductivity to heat in about the same proportion. The resistance to combustion and to the action of alkalies is considerably increased. The density of the untreated carbon of 0·55 inch diameter was 1·98, and after treatment was increased to 2·6. The proportion of carbon converted into plumbago was in this case 85 per cent. of the whole. These results, however, varied with the treatment. The carbon so treated is being used in arc-lamps, for the brushes of dynamos, and as electrodes in the electrolysis of alkaline salts.

The Author gives several examples of advantages gained by the use of this prepared carbon.

R. W. W.

The Explosion of a Liquid-Carbonic-Acid Receiver.

By S. Perissé.

(Mémoires et Compte rendu des travaux de la Société des Ingénieurs civils de France, July, 1895, p. 81.)

This explosion occurred on the 28th of December, 1894, and resulted in the death of a workman. The Author carried out the inquiry into the circumstances and causes of the accident, and this Paper is practically his report on the explosion.

The first part of the Paper is devoted to a consideration of the circumstances of the accident, and describes in detail its effects; it took place in one of the sheds of the "Carbonique Française" in which a large quantity of cylinders were stored. The Author

next considers the cause of the explosion, and goes very fully into the question of the strength of the faulty cylinder, which was of welded iron, made in Germany, and gave way by splitting longitudinally close to the inside edge of, but not through, the weld. He concludes that although the metal was somewhat reduced in thickness along the inner edge of the weld, the strength even at this point was sufficient for ordinary pressures; and he points out that the pressure in the cylinder might vary from the following causes: (*a*) the pressure of filling; (*b*) the degree to which the cylinder was filled, and (*c*) differences of temperature.

On the first point he found that the pressure of the pumps could and did vary from 60 and 90 atmospheres up to 120 atmospheres, but this variation by itself, was, he considered, insufficient to account for the explosion. On the second point, he ascertained that the cylinders were supposed to receive a fixed weight of acid, but though the apparatus was sufficiently sensitive when the receiver was not connected to the filling tube, after making this connection the balance was not so sensitive, and would allow of as much as 0·4 lb. being added to the scale, before it moved decidedly. With reference to the volume of the cylinders, on testing ten, he found a variation of capacity from 640 cubic inches to 683 cubic inches. Lastly, as to variation of temperature, it would appear that the cylinder was charged at a temperature of 2° C. and probably remained at or below this temperature till the morning of the explosion. On this morning, however, a stove was lighted in the shed, a short distance from the cylinder in question, and its temperature probably rose from 15° to 25° C. Taking these various facts into consideration, and also remembering that, owing to the slightly reduced thickness of the metal near the weld, the test pressure of 3,670 lbs. per square inch may possibly have strained the metal beyond the elastic limit, and thus have weakened it, the Author concludes that the explosion was due to the cylinder having been filled to excess at a temperature of 2° C. and then heated to about 15° C.

Finally the Author states the precautions which, in his opinion, should be taken to prevent similar explosions in the future. The cylinder should preferably be of solid drawn mild steel; it should be stamped with its weight empty, and also with the maximum weight of its charge of acid, this being based on the actual cubic capacity of the cylinder. The weight of the acid composing the charge should be checked by a method of double weighing; the test pressure should not exceed 2,500 lbs. per square inch, and the corresponding stress on the metal should be less than $\frac{7}{10}$ of the elastic limit; and the cylinder when charged should be carefully protected from changes of temperature.

The Paper is accompanied by several explanatory cuts in the text and a curve diagram, giving the corresponding densities, pressures and temperatures of carbonic acid between the densities of 0·0036 lb. and 0·036 lb. per cubic inch.

R. B. M.

The Sinking of the Vicq Pits of the Anzin Company.[1]

By — Saclier and — Waymel.

(Bulletin de la Société de l'Industrie Minérale, 1895, p. 27.)

For the purpose of working the south-eastern portion of their concession, the Anzin Mining Company proposed to open a new mine at Vicq in the valley of the Scheldt, where considerable difficulties were to be anticipated in sinking through the tertiary and cretaceous strata overlying the coal measures, which are essentially of a loose and water-bearing character; as the depth to solid ground was likely to be more than at the neighbouring Thiers pits, where it was necessary to put in 78½ metres of tubbing, and the sinking, which involved pumping 9,000 gallons of water per minute led to an expenditure of £220 per yard and twenty-six months of working time.

The two pits are of unequal size, a smaller one of 3·65 metres diameter being intended for the accessory work of pumping, ventilation, and travelling, while the larger one of 5 metres (16½ feet) in the clear is to be fitted with cages carrying eight tubs and capable of raising 300,000 tons of coal per annum. This size being considerably in excess of anything yet accomplished by the Kind-Chaudron process, it seemed too great risk to try it; but a more formidable objection was offered by the great thickness of loose ground near the surface, which would have required temporary protection by an iron lining tube, at a cost of not less than £4,000, until the permanent tubbing could be fixed.

The Authors were therefore desired to investigate Poetsch's method, in the different cases where it had been employed in Germany, Belgium and France (particulars of which are given in pp. 32 to 39), with the result that it was finally adopted for the new pits.

The section of the ground as determined by a preliminary boring in 1892 was as follows:—

	Metres.
Fine sand and gravel (alluvial)	6·75
Compact argillaceous sandstone (tertiary)	4·00
Chalk, loose and incoherent	19·95
„ compact	42·30
„ marl with flints	18·00
Blue marls	25·00
Plastic clay (*Dièves*)	58·00
Greensand	13·65
Coal measures reached at	187·65

These measures are all water-bearing down to 91 metres below the surface, where a secure foundation for tubbing could be found in the blue marl below the flinty chalk. Flowing springs are

[1] The original of this Paper, extending over 137 pages, with 14 plates, will be published separately in an enlarged form.—Sec. Inst. C.E.

encountered at two levels. The upper one, in the superficial deposits above the tertiary sandstone, depends mainly on the rainfall, and is capable of supplying about 2,700 gallons of water per hour, which, being sensibly alkaline and only slightly calcareous, is suitable for providing the steam required in the active working of a colliery of the largest size. The second water-level is encountered in the loose upper chalk; the water, being under pressure, rises and overflows to a height of 2½ feet above the ground.

In order to provide the feed and condenser water for the freezing plant, amounting to 160 cubic metres (3,500 gallons) per hour at 11° C., a well was sunk to the chalk at a distance of 250 metres from the site of the pits, whence the necessary supply is drawn by a centrifugal pump. This well, though only 2 metres in diameter and 15 metres deep, cost £400, the last 15 feet having been bored while the upper part was secured by an iron cylinder with a cutting shoe. The Authors think the work could have been done more cheaply by Triger's method of sinking by compressed air.

The borings intended to receive the circulating pipes for the freezing process, which were done by contract by Messrs. Hulster Brothers, are thirty-six in number, of a total length of 3,312 metres, twenty of them being arranged in a circle 6·5 metres diameter around the larger pit and sixteen on a 5·10-metre circle for the smaller one, all being of the same depth, 91 metres. The distance of the pits apart is 37 metres. In order to prevent the movement of currents of water through the ground, the springs were trapped in each of the holes at the different levels, the upper one by cementing the ground and the lower one at 9·75 metres by a close lining tube of 260 millimetres diameter, continued above the surface to a greater height than the hydrostatic head so as to prevent overflow, about twenty days being allowed to elapse before resuming the boring to allow the concrete to harden.

The freezing circuits, which are probably the most important elements in the whole plant, consist of a series of steel pipes, of unequal diameters, the smaller ones, of 30 millimetres diameter and 4 millimetres thickness of metal, being placed concentrically within the larger ones, which are 116 millimetres bore and 7 millimetres thick, each series being connected by goose neck to its own ring-main. The chilled fluid from the freezing machine passes from one of the rings down the inner tubes and returns through the outer ones to the surface and back to the refrigerator through the other. Extreme care was taken in the manufacture and fitting of these tubes, which were made, by the Escaut and Meuse Works at Anzin, of mild steel with 30 per cent. elongation before fracture, each length of 91 metres being set up and tested to 20 atmospheres pressure before leaving the works. The ring-mains are of 200 millimetres in diameter. The cold fluid moves at a speed per second of 10 centimetres in the smaller and 1·35 metre in the larger pipes.

The theoretical considerations involved in determining the thickness of the wall of ice and the choice of the freezing machine are given at considerable length on pages 49 to 62, the calculations following

a theory developed by Mr. Lebreton; the final result being that the work represented the abstraction of 110,000,000 calories from the ground about the large and 90,000,000 from the small pit, or, allowing 25 per cent. for losses, a total of 250,000,000 of calories as the total heat to be removed. As the amount of fluid circulating amounted to 4·250 kilograms per hour and its temperature was raised 2·5° C., the heat to be abstracted corresponded to 250,000 calories per hour, and the time required to freeze the ground 1,000 hours, or about forty days.

The freezing machinery on Linde's system consists of four double-acting cylinder-compressors, 360 millimetres diameter and 540 millimetres stroke, grouped in pairs and coupled at right angles to a shaft carrying a 5½-metre pulley, which is driven by a belt from the fly-wheel, 6·4 metres diameter, of a pair of coupled horizontal condensing engines, with cylinders of 580 millimetres diameter and 1,100 net stroke. The compressor-pistons work with a clearance of ½ millimetre in front and 1 millimetre behind, the latter quantity being larger to meet a possible elongation of the piston-rods. The cylinders are without cooling jackets, the heat developed in compression being absorbed by allowing a small quantity of liquefied ammonia to enter with the gas on the intake stroke. The compressed ammonia is liquefied in a pair of worm tubs, 2¼ metres in diameter and 3 metres high, each containing seven coils of 80-millimetre steel tubes 140 metres long, or 980 metres in all, giving a total cooling surface of 92 metres inside and 117 metres outside, the condensing water being supplied by two Burton diplex steam-pumps, each having two plungers of 178 millimetres diameter and 254 millimetres stroke. The cold water drawn from the feed-well enters at the bottom and overflows at the top of the tubs. It is afterwards used as injection water for the main engines. The two refrigerators, in which the liquefied ammonia is reconverted into gas by heat abstracted from the calcium chloride solution at its higher temperature, are similar in construction to the condenser; the tubs, of 2·4 metres diameter and 3 metres high, having eight coils of serpentine pipes, with a total length in each of 1,106 metres of tubes with a surface inside of 104 and outside of 132 square metres. In order to promote contact between the fluid and the tubs a set of rotating stirring paddles is added in each of the refrigerators. The chilled solution is drawn off at the bottom of the vats by a pair of Burton steam-pumps with plungers of 260 millimetres diameter and 254 millimetres stroke. The suction-pipe is common to both engines, but each has its own delivery, one being in connection with the circulating tubes of No. 1 pit and the other with No. 2. The return solution, heated by passing through the circuits, enters the refrigerating tub at the top, giving the same methodical application as in the condensers. The ammonia vapour produced returns by an 80-millimetre pipe to the suction of the compressors. A special series of rectifiers is interpolated between the compressors and the condensers in order to facilitate the separation of oil carried over by the gas, which might otherwise form

deposits in the cooling tubes and so diminish their effective power. The compressors were arranged so that they could be worked singly, or in groups of two, three or four together. The whole number were only at work together for about one-half the total time of the freezing, or twenty-four days out of forty-eight days, only two or three being used on the other days. The total quantity of ammonia in use in the apparatus was 732 kilograms, which was supplied by Mr. J. Peintre of Verviers at the price of 4·25 francs per kilogram. The calcium chloride solution in circulation measured 62 cubic metres and contained 25 tons of the dry salt, also supplied by Mr. Peintre at 150 francs per ton. The density of the solution was 1·25, corresponding to a specific heat of 0·68 per kilogram, or 0·85 per litre.

The progress of the operation of freezing was followed by a series of thermometers placed in steel tubes filled with a strong solution of calcium chloride and driven into the ground to a depth of 2 metres, forming a ring of 1 metre larger radius than that of the freezing circuits about each pit, with an additional series of fourteen spaced 33 centimetres apart on a line extending to a distance of 8 metres from the centre of No. 1 pit. These thermometers, which were carefully protected by non-conducting covers against absorption of surface-heat, were read daily, so as to be able to detect any irregularity in the working of the tubes which would have been indicated by any marked differences in the readings of adjacent thermometers similarly placed. Provision was made to meet this by placing an independent feed regulator on each of the circuits, but this was not used, as the work went on regularly throughout, owing to the large section of the distributing ring-mains.

The freezing-machine was started with one compressor on the 28th of May, 1894, the ground temperature being then 11·65° C., the cold developed being represented by 285,000 units per hour. The next day the temperature of the solution had fallen to $-47°$ in the refrigerator and $-1°$ in the return pipe; a second cylinder was then started, going from 250,000 to 275,000 units, which reduced the temperature of the solution to $-7°$, and $-4°$ on the third day, when the third compressor was started, and in ten days more $-10·6°$ was obtained in the cold solution. Afterwards the whole set of four compressors was worked from the 12th of June to the 1st of July, the temperature being then $-15°$ in the supply-pipes.

At this date the freezing of No. 2 pit was complete, and thence forward only two compressors were used until the 17th of July, when the initial freezing operations may be considered to have been completed. The thermal equivalents of the work done during this period were computed to be as shown in the Table on the following page. Subsequently the ice walls were maintained during the sinking of the pits by working three or two compressors, the latter number having been in use from the 2nd of September. On the 1st of December, the machine was stopped during the night, as well

as on Sundays and holidays, and on the 28th of December its use was entirely discontinued after seven months' service, the total stoppages during that time being only seventy-six and a half hours.

Heat absorbed in	Pit No. 2.	Pit No. 1.
	Calories.	Calories.
Formation of ice	43,040,000	70,075,200
Cooling-ground outside circuits	16,825,615	28,285,845
„ „ inside „	14,473,982	22,917,860
Cooling work utilized	74,339,597	121,278,905
Surface losses	25,574,640	32,793,936
	99,914,237	154,072,841
Work done by engines, May 28 to July 2 .	100,379,694	..
„ „ „ July 16 .	..	161,354,901

The sinking was begun in the smaller pit on the 2nd of July, and in No. 1 on July 16th, the ground being then frozen in a ring of 1·20 metre thick, extending 45 centimetres outwards from the freezing centres and 75 centimetres towards the centre in the first case, and 55 and 100 centimetres respectively in the second. By keeping the source of cold entirely outside the ground to be excavated, a very large proportion of the latter was left in a loose condition and could be excavated by shovelling, and it was only necessary to break down a small portion of hard ground, which was done by shearing it back with picks and taper-pointed bars and wedges, the use of explosives being entirely avoided. This result was due to the abandonment of the former method of placing freezing circuits inside the pit so as to freeze the whole ground across, when the sinking must be conducted in the same way as in ordinary hard ground. The thickness to be cut depended upon the proportion of water in the strata, which is estimated to have varied from 50 per cent. in the first 28 metres to 25 per cent. in the remaining 63 metres, and was least in the more watery ground on account of the larger quantity of ice to be formed per unit of volume. The most favourable results as regards rapidity of sinking were obtained in the upper soft part of the chalk, which was in the condition of a thick mud, with only about 16 inches of the thickness of the ice-ring, within the area of the smaller pit, and 8 inches in the larger one. The more compact chalk below was also rapidly gone through, owing to the prevalence of a system of vertical fissures, which allowed it to be readily broken by hammers and wedges. In these beds the maximum rate of sinking of 2½ metres and 2 metres per day was attained, but the speed diminished in the more silicious strata below, and in the flinty beds it fell to 30, 40, and 50 centimetres per day, a whole month having been required to get through a thickness of 12½ metres. During this period the temperature in the pit

was − 12° C., the lowest point reached in the sinking. The diameter of the unfrozen portion of these beds was only from 1·65 metre to 1 metre in the larger pit, which never froze completely across, but in the smaller one it was entirely frozen, and had to be broken down by wedges under circumstances of great difficulty, the number of picks and wedges blunted in a day having been as many as 3,000 in No. 1 pit. The blue marls below the flinty beds were entirely unfrozen, no ice having been met at a greater depth than 85 centimetres below the bottom of the freezing circuits in either pit, and the sinking was then continued in the ordinary way. The actual time required for sinking in the frozen ground, was from 2nd July to 5th October in the smaller, and from 16th July to 16th October in the larger pit, which includes about nine days of stoppages for building the first section of tubbing. This consists of twenty rings of 25 millimetres thickness of metal, extending from two wedged curbs at 30·70 metres in depth to the surface. A second line of 62·6 metres total height, formed of forty-four rings tapered in thickness from 30 millimetres to 45 millimetres stands upon curbs bedded in the marls at 93·30 metres, and a third of fifteen rings of 50 millimetres thick, carried by three curbs in the gault (*dièves*), completes the whole height of 117·65 metres. The tubbing is of the ordinary segmental kind, the only difference being in the presence of lugs and sockets for the cross-bearers carrying the guides and other inside fittings for the cages. The rings are set with a backing of about 8 inches of concrete, made of 2 parts of hydraulic lime to 3 of burnt shale, except in the two lower rings of each series, where a stronger mixture, of equal parts of Portland cement, lime, and burnt shale, is used; the mixing was done with water containing 10 per cent. of calcium to prevent it from freezing. The joints between the different lengths of tubbing are closed by double rings of oak wedged tight in the usual way.

The cost of the sinking is given in very full detail on pages 130–140, the total amount having been about £28,400, or £120 per metre, the different items being summarized in the following Table:—

——	Per Cent.	Total.	Per Metre.
		Francs.	Francs.
Patentee's royalty	4·6	32,760·00	139·20
Temporary plant and buildings	2·7	19,582·40	83·25
Borings for freezing-tubes	10·4	73,673·03	313·10
Freezing-plant	35·0	248,765·56	1,057·20
Measuring-apparatus	0·3	1,898·68	8·10
Freezing	4·7	33,030·95	140·40
Sinking and tubbing	40·5	287,454·77	1,221·65
Carriage	0·6	4,562·00	19·40
Tools	0·7	5,257·00	22·35
Sundries	0·4	2,865·00	12·15
Total	99·9	709,849·39	3,016·80

Supposing the whole of the plant to be charged to the single use, its subsequent employment in future sinkings would benefit them to the extent of about 1,000 francs per metre, so that the work could be done for about 2,000 francs per metre. The items specially chargeable to the freezing-operations are—

	Francs.
Patent rights	32,760·00
Boring	73,673·03
Erecting	14,084·72
Measuring instruments	1,899·68
Freezing-cost	33,030·95
Total	155,448·38

This sum, corresponding to about 660 francs per metre, represents all that would have been available for bearing the cost of pumping, temporary lining, and the numerous other charges incidental to sinking in heavily watered ground, supposing the ordinary method of sinking had been adopted. The work done and the utilization of the cold produced by the machines have been the subject of numerous experimental measurements; the reductions of the observations being summarized in a graphic form in one of the accompanying illustrations, Plate XII. The coal burnt in supplying steam for the compression was 2,191 tons between the 28th of May and the 28th of December, which, allowing for short time and stoppages, corresponds to about 200 working days. The work developed corresponds to a total removal of 851,656,686 calories, or 461,353,162 calories for the larger and 390,303,706 calories from the smaller pit. About 20 per cent. of the cooling effect was lost at the surface owing to the distance of the machines from the pits. This was done as a matter of convenience, as it was intended to use the engines for driving air-compressors in the workings of the mine, and they were therefore erected on permanent foundations; but, as the Authors point out, it would have been better to have placed them nearer to the work. They also think that when two pits are being sunk at the same time, each should have its own freezing plant, and that the work should be begun as soon as an ice wall of sufficient thickness has formed over the surface, so as to be able to have sufficient soft ground to get down rapidly. With a second set of machines, able to replace each other to some extent, they would not hesitate in sinking down to the level of the bottom of the tubbing without any intermediate support, so that it could be built up to the surface in a single column without requiring any intermediate wooden joining rings, which must always be a source of weakness to the lining of the pit.

It is so important that the borings for the freezing should be perfectly vertical that the Authors consider that this condition should be strictly insisted upon in the specification when the work is done by contract by a fine of double the price for any

crooked hole. This would be no hardship, as the slightest deviation from the vertical is readily detected by the master borer when the work is going on. The maximum distance between the holes should not be more than 1·20 metre when the depth of ground to be frozen is not more than 100 metres, but for greater depths it would be well not to exceed 1 metre.

H. B.

The Burton Liquid Electric Forge. By W. W. Ker.

(The Electrical Engineer, New York, 1895, p. 53.)

The method of heating adopted in this forge depends on the rise in temperature of the negative electrode which takes place when the hydrogen gas liberated during the decomposition of water takes fire.

Heating by means of immersion in water, was first carried out practically by connecting a lead plate to the positive conductor and a pair of tongs to the negative. The metal to be heated was then grasped in the tongs, and immersed in a vessel containing the positive plate and filled with dilute sulphuric acid or a solution of soda. The material gradually heats, and, if kept in long enough, melts; the article remaining in view during the whole operation can be withdrawn at the desired moment.

This method involves a very heavy consumption of current, but the principle referred to above affords the means of reducing it materially, the heat generated by the combustion of the hydrogen being absorbed by the iron. In this process, the positive pole is connected to a lead plate suspended from the edge of a rectangular wooden tank, having a float worked by a handle and rackwork which admits of the surface of the liquid being raised or lowered at will. The negative conductor is attached to three work-rests, two of which are used for heating articles which can be held in tongs, the third for heating particular points in a long bar. In the latter case, the bar is laid in a trough at one end of the tank on two movable pieces of firebrick, the space between which determines the length to be heated, which may vary from $\frac{1}{4}$ inch to the full width of the tank. Contact is made between the iron and the work-rest by means of clamps. With this apparatus, the iron is merely brought into contact with the surface of the liquid, hydrogen is given off and takes fire. The circuit is completed by raising the surface of the liquid by depressing the float, and when articles of irregular shape are being dealt with, contact is made and broken many times, so as to give the heat time to become distributed.

The liquid employed is a solution made by dissolving 10 lbs. of washing soda and $1\frac{1}{2}$ lb. of borax in 45 gallons of water. The time required for heating varies from eight seconds for a piece of iron $\frac{1}{4}$ inch square to one minute for a piece 1 inch square; to heat

the latter 26 amperes at 220 volts are consumed. The cost is stated to be considerably less than with the ordinary method of heating with coal, and better welds are said to be made with the electric forge, on account of the absence of scale or oxide on the heated metal.

A plant of the kind described is at work at the Niagara Falls, where current is supplied at a pressure of 500 volts, and an application of the process to the reduction of nickel is in operation in Canada, the ore being placed in a cradle, and the circuit being completed by allowing water to come in contact with it.

C. H. W.

The Influences of the Air-Gap on the Characteristic Curves of Dynamos. By E. J. Brunswick.

(L'Eclairage Électrique, vol. iv., 1895, p. 411.)

The Author, after referring to the great assistance which the drawing of the characteristic curve connecting the excitation and the electromotive force of a dynamo is to the designer, proceeds to discuss the question of the influence of small modifications in structure on such curves. These curves are usually obtained by running the machine at constant speed and observing the variation of the electromotive force on open circuit with alterations in the exciting ampere turns. In such conditions the ordinate of electromotive force is proportional to the magnetic flux. The curve thus obtained may be divided into two parts. In the first portion, this curve is straight, and the magnetising force is practically all required to overcome the resistance of the air-gap. In the remaining part of the curve, which is situated above the knee, the influence of the iron as it approaches saturation is seen. The Author, by reference to figures, shows that the inclination of the first portion of the curve is directly proportional to the length of the air-gap. He then proceeds to develop from the original characteristic a new curve for the same machine with an increased air-gap. He obtains by geometrical analysis the two components of the magnetomotive force for any point P on the original curve. The one component is that required by any resistance of the air-gap, and the other is due to the resistance of the iron circuit. The new point P′ on the curve for the increased air-gap is then obtained by the inverse operation.

The Author admits that the reasoning will not be absolutely correct if the armature reaction is very much affected by the alteration in the air-gap. He finally treats in the same way the alteration in the characteristic curves when current is flowing. The article is illustrated.

R. W. W.

The Relations between Pressure, Electrical Resistance and Friction in Brush Contact. By E. V. Cox and H. W. Buck.

(The Electrical Engineer, New York, vol. xx., 1895, p. 125.)

The Authors' object was to ascertain the power absorbed by dynamo-brushes under different conditions. Particulars are given of the apparatus employed by means of which the resistance of brush-contact could be determined and the pressure of the brush and tangential pull due to the friction measured. The brushes were caused to press either on a commutator or metal cylinder mounted on the shaft of a 1-HP. motor.

A number of curves are given, having for ordinates the electrical resistance and tangential pull and for abscissas the pressure on the brush. Each curve given is the mean of four similar ones agreeing with one another within a few per cent. The curves relate to a radial carbon brush on a cast-iron pulley and on a commutator; two different peripheral speeds being employed in each case; also to a carbon brush placed tangentially. Other conditions investigated were: a copper brush bearing tangentially on a commutator and on two cast-iron pulleys of different diameters running at different peripheral speeds; the effect of oiling a commutator having a tangential copper brush bearing on it; the resistance per square inch of contact of a copper brush and a radial and tangential carbon brush on a cast-iron pulley; and of the same brushes on a commutator.

The following are among the Authors' conclusions:—

(i.) Beyond a certain point, a great increase in pressure only slightly diminishes the resistance, whereas the tangential pull is directly proportional to the pressure.

(ii.) The contact resistance of carbon is much higher than that of copper, but the friction is less with a carbon than with a copper brush.

(iii.) The contact resistance, and also the friction, of all brushes is less on a cast-iron pulley than on a copper commutator.

(iv.) Slightly oiling the cylindrical surface only slightly increases the resistance and greatly diminishes the friction.

(v.) The friction of a radial carbon brush is greater than that of a tangential one at the same pressure. The friction of all brushes is slightly less at high than at low peripheral speeds.

C. H. W.

Glow-Lamps and Motors on the same Circuit.

By E. Hoegerstadt.

(Elektrotechnische Zeitschrift, 1895, p. 185.)

The Author describes and illustrates with diagrams the method which he adopted in carrying out an installation of electric light and power for a Swiss rope-factory. The source of power (a distant waterfall) made it necessary to use a high electric pressure, but the small output involved did not justify the use of alternating currents; accumulators also would have been inefficient, although desirable in order to steady the current for the glow-lamps, the motors being used very irregularly. A total of 186 glow-lamps had to be installed with two motors, one of 6 HP. and one of 20 HP. The Author decided eventually to put down two generator dynamos in the turbine-house, each giving 61·3 amperes at voltages of 525 and 157·5 respectively, their efficiencies being 92 per cent. The circuits were so arranged on the three-wire system that the small motor and part of the lamps lay between one of the outer conductors and the middle wire (which may be called circuit A), whilst the large motor and the remaining lamps were fed through the other outer main and the middle wire, called circuit B. In the former circuit, the glow-lamps—thirty-one in number, arranged in the offices—were all separately connected to the mains, being constructed for a voltage of 150; the small motor was also designed for the same pressure—a much more convenient one than 500 or 600. In circuit B, on the other side of the middle wire, 155 lamps (for the factory) were coupled up in thirty-one rows of five lamps each (100 volts) in series; and the large motor was also designed for the same voltage, *i.e.* 500, between the mains. A special double-contact switch was arranged with each machine, by means of which a corresponding resistance could be put into the circuit when either machine was thrown out as its work ceased. Taking 2 per cent. drop in the lamps, 5 per cent. in the conductors, and 8 per cent. in the generators, 14 per cent. loss in the small motor and 10 per cent. in the large one, a total useful effect of 83·1 per cent. was obtained, and the Author thinks this is a notable result, seeing that accumulators were not employed.

F. B. L.

The Electricity Generating Station of Ardières, near Beaujeu, France. By P. Biles.

(L'Industrie Électrique, 1895, p. 356 *et seq.* 3 Figs.)

This generating station is driven by water power, and is situated near the source of the River Ardières, about 3·7 miles up stream from Beaujeu. Turbines were to be installed to obtain about 200 HP. from the stream.

The machine-room was built to accommodate two turbines of 100 HP., each driving directly two alternators of the same output. At present only one turbine and one dynamo are put down. The speed is 400 revolutions per minute, and a current of 20 amperes at 3,500 volts is produced with a frequency of 60 periods per second. The turbine was built by Messrs. Faesch and Picard of Geneva, and the alternator with exciter upon the same frame was built at the Oerlikon Company's workshops. The installation was put down to supply electric light to the town of Beaujeu, about 3·7 miles away, but owing to the terms of the concession given to the gas company this project had to be given up temporarily, and it was decided to supply current to Cercié, Belleville, and Montmerle, distant respectively 8·7, 12·4, and 15 miles from the station.

The potential of 3,500 volts was insufficient unless very heavy conductors were used, a transformer was therefore employed to raise the potential to 10,500 volts.

The Author then refers to the switchboard, which seems to present no special features of interest. A line will eventually serve Beaujeu at 3,500 volts.

The 200 HP. available will be divided, one-half going to Beaujeu, and the other half to the towns already named. The loss calculated for upon the lines is 10 per cent. For Beaujeu a copper conductor 0·174 in diameter will be used, and for Belleville one of 0·118 in diameter, while between Belleville and Montmerle this is reduced to 0·079 in diameter.

Since work began a number of the insulators have broken down and caused short circuits.

The insulation of the line on certain fine days has been 12,000,000 ohms, but in bad weather with wet and snow it has fallen to 100,000 ohms.

In either case the electric lighting was quite satisfactory, but short circuits have occurred on the primary when the insulation fell below 75,000 ohms.

The Author considers, however, that the system is defective and that it would be better to have two totally distinct lines hung on the same poles.

A telephone with complete metallic circuit puts the station at Ardières in connection with all the distributing centres.

The transformers are of the Labour type made by the "Société l'éclairage electrique." They are in air-tight metal cases surrounded by paraffin oil.

When the line is fully loaded the potential at the transformer terminals at Cercié will be about 9,800 volts, at Belleville 9,600, and at Montmerle 9,450 volts.

The secondary of the transformers feeds three-wire circuits having a potential of 240 volts between the outside wires.

At present there are installed at Cercié one transformer of the 4 kilowatt size; at Belleville four transformers each 10 kilowatts.

A transformer compensator is used in the station to maintain a constant potential at the distributing centres. The Author then

gives particulars of the action of this transformer. He also points out the difficulty which will ensue if Beaujeu is to be supplied with current at 3,500 volts while all the other places are supplied at 10,000 volts. In his opinion it would be far better to use the higher potential for all the distributing points.

This is stated to be the first case of distribution of current at 10,000 volts in France, and the Author expresses his satisfaction that all the apparatus has been produced in France.

E. R. D.

The Paris Municipal Regulations for Electrical Installations.

By — Poubelle.

(L'Électricien, 1895, p. 140.)

The regulations dated the 3rd of July, 1895, and issued by the head director of works, contains sixteen clauses concerning the requirements of the council, to be observed in laying down installations in the city of Paris. The following are the outlines of the new regulations.

Clause I. All installations supplied by a concessionee of the city of Paris must satisfy the following conditions at all times:—

Clause III. The cross section of the metallic conductors must always be such that the accidental passage of a current of double the normal intensity shall not result in a temperature rise of more than 40° C. In every case the current density must not exceed 1,940 amperes per square inch section for wires ranging in size from Nos. 19 to 13 B.W.G., nor more than 1,300 amperes per square inch for wires having sectional areas from 0·0078 square inch to 0·078 square inch, and for wires of larger size the density must not exceed 650 amperes per square inch. These figures are for insulated wires, and with bare wires the densities may be doubled. No conductor may be used the core of which consists of a single wire of a smaller diameter than 0·040 inch.

Clause IV. Outside the switchboards all cables and wires must be both insulated electrically and protected mechanically. The nature of the insulation must be such that it has no chemical action on the copper core.

Clauses V. and VI. deal with the design of fuses and switches, and the permissible temperature these may attain.

Clause VII. states that a fuse and switch shall be placed at each point where a branch wire leaves the main, and that double-pole fuses shall be used if the sub-mains are attached direct to transformers.

Clause VIII. details special precautions when dynamos, transformers, or accumulators are used in the installation.

Clause IX. Care must be taken that each part of the installation can be easily distinguished and isolated from the rest in case of

need. To this end each circuit shall be provided with a double pole fuse, and the same shall also be used in any branch in which the current exceeds 5 amperes.

Clause X. deals with the means employed to fix and support the cables, and in certain cases tubes are advised.

Clause XI. In jointing cables no acids shall be used or any material likely to damage the metallic core, or the insulating substance.

Clause XII. also deals with wood casing, and states that in no cases are metallic staples to be used to secure the wires.

Clause XIII. states that the greatest care must be exercised in the wiring of electroliers, brackets, &c. The conductor used in them must have high insulation resistance. In electroliers having five or more lamps, all joints must be avoided, and the wires of each lamp should be preferably connected on to metallic plates by means of screws.

Clause XIV. deals with arc lighting and the rheostats required in this work.

Clause XV. When in the same installation both gas-pipes and electrical conductors are used, the following special rules must be observed: (*a*) The fittings serving both for gas and electricity shall always be mounted on a base plate whose insulation resistance shall be at least 500,000 ohms, and which shall be so arranged that this insulation shall not be impaired with dust or damp; (*b*) The wires attached to these fittings shall be well insulated and protected from the heat of the gas.

Clause XVI. deals with the method of determining the insulation resistance allowable. If E is the potential in volts at the terminals of the generator supplying the installation, then the insulation resistance of every branch circuit which can be isolated from the rest of the conductors by the removal of a double-pole fuse, shall not be less than 5,000 E. The measurement of the insulation resistance shall be made with the normal voltage used, provided this voltage is within the two extremes of 100 to 500 volts.

R. W. W.

The Electric-Lighting Station at Hanover.

(Elektrotechnische Zeitschrift, 1895, p. 150.)

This article is practically abstracted from a very complete and elaborate report prepared by Dr. O. Gusinde on the results of the third year's work at the Hanover Electricity-Supply Station. The total capacity of the plant is equal to an output of 800 kilowatts. Four water-tube boilers are installed, three of which have each a heating surface of 1,900 square feet, and a grate surface of 45 square feet, whilst the fourth is of a larger capacity, having a heating surface of 3,260 square feet, and a grate surface of 75 square feet. The working steam-pressure is 170 lbs. per square inch.

The boilers—during the year in question—were at work altogether for 4,375 hours, or an average of twelve per day; the total coal burnt was 991 tons, of which about 20 per cent. was used for raising steam to full pressure. Anthracite was employed exclusively, at an average price (delivered at works) of 13*s.* per ton. From 7 lbs. to 8 lbs. of water were evaporated per lb. of coal; the average amount of ash and cinder was 9·2 per cent. The engines installed are three in number, all of the vertical type, triple expansion, with jet condensers. Two of them develop up to 400 HP. at 145 lbs. pressure in the steam-chest, and running at 120 revolutions per minute. The third indicates up to 600 HP. The smaller engines are each coupled direct to a flat-ring dynamo of the Schuckert type, having an output of 250–275 kilowatts; and the large one to a similar machine of 400 kilowatts capacity. These combined generator-plants were in operation during the year for 2,520 hours, or an average of 6·9 hours per day; the kilowatt-hours were 491,150, or a daily average of 135.

Taking the efficiency of the dynamos as being 90 per cent., the effective horse-power-hours of the engines—740,000—show a total coal-consumption of nearly 3 lbs. per horse-power-hour, or about 2¼ lbs., deducting coal used for lighting up, steam raising, &c. Including oil, waste, &c., the cost per kilowatt-hour works out to very nearly 2½*d.* Accumulators are employed in this station, and during the year were drawn upon for rather more than a third of the total ampere-hours required; the daily average output in ampere-hours was 3,670, or 69 per cent. of the accumulator capacity. The maximum (28th December) was 6,410, or 122 per cent.; the minimum (31st August) 1,590, or 30 per cent. Of the total output from the station in kilowatt-hours, 20·2 per cent. was lost. The accumulators accounted for 8·4 of this waste, and the mains with distributing networks, &c., for the remaining 11·77. The individual consumption is shown in the annexed Table:—

Description of Consumer.	Number.	Percentage of Total Consumption.	Relative Consumption (in Kilowatts) per Consumer.
Shops	267	39·2	1·36
Hotels, restaurants, &c. . . .	32	15·0	4·33
Banks and offices	54	10·1	1·73
Theatres, music halls, &c. . .	7	4·5	5·91
Private houses	26	4·6	1·65
Churches, schools and museums	4	4·1	9·38
Works, warehouses, &c. . . .	13	8·2	5·82
Streets	2	2·2	10·35
Motors	26	10·8	3·85
Electricity-supply station . .	1	1·3	11·60
	432	100·0	{average 2·14

Aron meters to the number of 438 are used. The total receipts were, for the year, £15,648, whilst the total payments (inclusive of interest on capital outlay at $3\frac{1}{2}$ per cent.) only reached the sum of £8,312. An amount of £5,336 was written off as an allowance for depreciation, leaving £2,000 as net balance for addition to reserve fund, &c. To the end of the year under review the total expenditure had been £106,402, of which £14,640 had been paid back from revenue by way of redemption, leaving an apparent capital outlay of £91,762. The reserve fund at the same time was nearly £4,000.

F. B. L.

The Electricity Works at Rotterdam. By L. Bruhnes.

(L'Eclairage Électrique, vol. iv., 1895, p. 489.)

The Author, who is an assistant engineer at the Electro Technical Institute at Liége, describes fully the recent system of electrical distribution established in this important port. The character of the town presents many interesting problems when designing the electrical system. The area is extensive, and is divided by the River Meuse. The width of the branches of this river where the island of Noordereiband is situated, are respectively 330 yards and 165 yards. The various quays require special arc-lighting services, and it was also proposed to work the numerous cranes for unloading ships electrically. Hence a high voltage and direct current were needed. The work has been carried out by Messrs. Siemens & Halske of Berlin, for the town authorities. The central station is situated in the gas-works, which are a good way from the centre of the town. Two Willans engines of 150 HP. are at present installed, and they drive two Siemens dynamos of the external armature type. The field rheostats and magnets are so designed that the voltage may be varied from 450 volts up to 700 volts. At the lower voltage the dynamos supply the network direct, and the 700 volts are required to charge the accumulators in the two substations. These are situated at a distance of about $1\frac{1}{4}$ and $1\frac{3}{4}$ miles respectively from the generating station. Owing to the marshy nature of the subsoil, the insulation of the cables needed special care. When crossing the Meuse armoured cables are placed in the bed of the river, as the bridges have swinging sections and hence could not be used. The distribution service from the substations is on the 5-wire system, with motor generators for equalising the pressure between the different wires when the load on any individual pair of wires varies. The total pressure across the two outside wires is regulated automatically by means of a motor varying the number of batteries in circuit.

The motor generator which for the 5-wire system must contain four sets of field magnets with armatures rigidly connected would be

cumbersome if all the armatures were in one line. An ingenious arrangement has therefore been devised of placing two such sets of two machines side by side, and synchronising them by means of three phase connections between two armatures of the adjacent sets.

The first substation is placed in the busy part of the town, and is used mostly for lighting purposes, while the second substation at the docks does a considerable amount of power work and arc lighting. There are at present four electric cranes lifting 1½ ton. Two motors are employed on each, *i.e.* an eight HP. motor for lifting and a four HP. motor for traversing purposes. The rheostats for regulating these are specially illustrated and described. Carbon contacts are used in them, as the action of the humid atmosphere was found to corrode metallic contacts. The Author concludes by complimenting the engineers on overcoming the essentially local difficulties. This article is illustrated by four blocks.

R. W. W.

The Use of the Alternating Current for Lighting Gedney's Channel, New York Harbour.

(The Electrical Engineer, New York, vol. xx., 1895, p. 101.)

The United States Lighthouse Board arranged some time ago to light Gedney's Channel, and the South-west Spit, by means of buoys carrying electric lamps. This channel is the entrance from the Atlantic to the bay within Sandy Hook.

The first plant was an experimental one, the continuous current being used, and was so successful that two 9-kilowatt machines have now been put down for permanent work; either will do the whole of the work, the other being a standby. These machines have four poles, and run at 1,200 revolutions per minute; they are provided with commutators and collecting rings, and will therefore furnish both alternating and continuous current, the former having a periodicity of 40 cycles per second, and being distributed at a pressure of 1,000 volts, the latter a pressure of 150 volts. Each machine has a 12-kilowatt transformer insulated with oil. The alternating current feeds the lamps in the Gedney Channel buoys and the Hook beacon, the continuous the South-west Spit circuit.

The generating station is almost at the extremity of Sandy Hook. Two pairs of conductors connect the station, one pair to the Gedney Channel cable, a branch being taken off to the Hook beacon, the other pair to the South-west Spit buoy. The conductors are laid underground in creosoted wood troughing; one wire of each pair is insulated with rubber protected by lead, the other being bare. The Gedney Channel submarine cable is 6·18 miles in length, and consists of seven No. 18 B. & S. copper wires, stranded together; the insulation is gutta percha, and has a

resistance of not less than 300 megohms per mile; the cable is armoured with hard drawn copper wires of the same gauge as the strands of the conductor, the diameter overall being $\frac{3}{4}$ inch. The submarine cable supplying the South-west spit is 1·93 mile long; the conductor is the same as in the last cable, but the armour consists of two layers of steel wire; the overall diameter is $1\frac{5}{8}$ inch, and the weight per mile 19,113 lbs.

The buoys consist of stout poles about 70 feet long, each being anchored by a heavy mushroom weight. A 600-watt transformer, contained in a watertight case filled with heavy oil, is fixed inside the head of the buoy, openings into the transformer being made watertight. The lamps fixed at the top of the buoys are 5 inches in diameter with spiral filaments, and give 100 candle power, taking $3\frac{1}{2}$ amperes to 4 amperes at 100 volts; they are protected by a bell glass globe. A similar lamp is fixed in the Hook beacon, and is fed from a transformer in the same manner. The alternating current also supplies the lamps in the station and the keeper's dwelling.

C. H. W.

Electric Lighting of the Baltic Canal.

(Elektrotechnische Zeitschrift, 1895, p. 378.)

To adequately light up the entire length ($62\frac{1}{2}$ miles) of the Baltic Ship Canal, including the sluices, harbours, buildings, lighthouses, locks, &c., two electricity stations have been erected, one at Holtenau, the other at Brunsbüttel, each adjacent to the hydraulic pumping depots which operate the sluices and lock-gates, the same boilers supplying steam for both purposes, hydraulic and electric. At Holtenau the electric-lighting plant consists of two horizontal tandem compound steam engines, with cylinders $15\frac{3}{4}$ inches and $24\frac{1}{2}$ inches diameter respectively and 40 inches stroke, making 85 revolutions per minute; these are coupled direct to two alternating-current dynamo generators with rotating field-magnets 15 feet 6 inches diameter. The periodicity is 102 per second, the normal pressure being 2,000 volts. Exciting current at a pressure of 120 volts to 150 volts is obtained from a four-pole continuous-current machine mounted on each generator axle. The exciter field-magnets are regulated automatically by means of a Tesla motor, which throws resistance in or out according as the transformed pressure in the engine-house rises above or falls below the normal amount, 72 volts. This regulation is effected with as small a current as 1 ampere. Each generator gives in normal work (steam-pressure 90 lbs. per square inch) 100 kilowatts, using about $27\frac{1}{2}$ lbs. steam per hour per kilowatt. One set at each station suffices to take the load, leaving the second set as a reserve.

For lighting the sluice-chambers, &c., in the daytime, a small combined engine and dynamo of 10 to 12 HP. is provided; this runs

at 150 revolutions per minute, the steam-cylinder being 6 inches diameter, 13¾ inches stroke. The dynamo is of the continuous-current four-pole type with disk armature, and serves also in case of emergency to give current for exciting the alternators. For lighting the canal itself, one of the stations supplies current to the north and south sides for half the length of the canal; the other stations for the corresponding lengths over the other half. There are thus four circuits, two being operated from each station. Each circuit feeds about 250 glow-lamps of 25 candle-power each, placed along the canal proper; their average distance apart is about 175 yards, but the spaces between lamps vary from 87 yards at the curves to 275 yards on straight portions. The overhead conductors supplying current to these lamps are of copper, 4 millimetres diameter (equal to No. 8 S.W.G.) mounted on porcelain insulators of the triple-bell type which are secured every 45 yards to wooden poles. At points where lamps are placed a shunt arrangement is provided, consisting of an iron core with windings put in parallel with the lamp conductors, and so designed as to take 9 per cent. of the normal current. It serves as a by-pass, in case of breakage or defect in the lamp itself; and as many as one-third of the lamps on each circuit might suffer accidental extinction before regulation of the current would be necessary at the power-house. Each lamp requires a pressure of 25 volts, so that for the complete circuit a pressure of 7,500 volts is provided, the current being transformed up to this from the generator pressure of 2,000 volts.

An electric launch, fitted with a 12-HP. motor and capable of traversing the entire length of the canal in less than eight hours, serves for inspection and repair work.

The details given are accompanied by several diagrams and illustrations.

F. B. L.

The Electricity-Works at the Dresden Railway Station.

By R. Ulbricht.

(Elektrotechnische Zeitschrift, 1895, pp. 401, 435.)

A very extensive and elaborate electricity supply station is in operation at Dresden for lighting the various railway depots in that city, also for lighting and power purposes in the locomotive works, repair shops, &c., belonging to the railways; and in this article is given a most complete and well illustrated description of the entire plant.

The boiler-house at the supply station contains five boilers (with space for a sixth), each having 1,615 square feet total heating surface. In the engine-room are four steam-dynamos (each of 300 HP. capacity or 220 kilowatts) with space for a fifth set having double this output. The engines are of the horizontal tandem compound condensing type, running at 100 revolutions

per minute with 120 lbs. steam-pressure; the stroke is 3 feet, diameter of cylinders 18 and 30 inches respectively. The dynamos, mounted on the engine axles, are of the rotary current or "drehstrom" type, with moving field-magnets and generating current at a normal pressure of 150 volts, the periodicity being 100. Exciters are mounted on the axles of the generators in the usual way. The exciters give each 182 amperes at 110 volts pressure. The "drehstrom" current is transformed up to 3,000 volts for transmission purposes, a bank of nine transformers, each of 100 kilowatts capacity, being erected at the supply station, with room for six more. Current for lighting the supply station itself is taken from the exciter mains, there being one 20-ampere and seven 15-ampere arc-lamps, with 20 glow-lamps of 16 candle-power each. Water for condensing purposes is pumped up by means of a "drehstrom" motor of 20 HP. capacity, fed direct from the generator mains.

From the supply station thus briefly described, current is derived for lighting and power purposes at the rate of about 60,000 and 12,000 kilowatt-hours respectively per month. The total outlay so far amounts to £87,500. Although spoken of as a three-phase system, the arrangement adopted for this combined power and lighting equipment is somewhat modified from the ordinary form of rotary current transmission. There are in reality two different circuits, inter-connected at the generator mains, for the lamps and motors respectively. The former are supplied with current from two of the generator windings, thereby being fed practically by an ordinary alternating current; whilst the motors, of the regular three-phase type, take current from all three windings, although the phases are not absolutely symmetrical, but relatively somewhat displaced. Special switch-gear of course is provided to allow any of the machines to supply alternating or three-phase current as required. In the supply station there are three small motors at work, having a total output of 23 HP., and one large one of 10 HP. Forty motors are installed in the locomotive works, having a total capacity of 150 HP. In addition to these it is proposed to instal five 10-HP. motors for dealing with freight and baggage, also eight of 40 HP. and eight of 7½ HP. each for the cranes. These motors are all of the three-phase type, taking current at 120 volts. Amongst the purposes for which they are used in the works are the following:—Locomotive transfer-tables; overhead travelling-cranes; ventilating-fan; grindstones; wheel-lathes; screw-cutting and surfacing lathes; planers, shapers, milling- and boring-machines; plate shears; punching-machine, &c.—in fact, most of the usual heavy tools employed in machine shops. These are in many cases driven by individual motors, but seven motors, ranging in size from 3 to 15 HP., are used to drive counter-shafting in various parts of the works. The average total simultaneous output of the motors is about 70 HP.

F. B. L.

The Electric-Lighting of Passenger Coaches on the Dortmund-Enscheder Railway. By — Staberow.

(Verhandlungen des Vereins für Eisenbahnkunde, 1895, p. 58.)

The Author recounts the different attempts made at electric lighting of trains by means of dynamos driven in various ways, points out the disadvantages attendant on their use, and shows that the cheapest and simplest way of producing the light is by the employment of accumulators placed under each coach and charged from dynamos used for the lighting of the larger stations.

After a number of exhaustive experiments the directors of the Dortmund-Enscheder Railway determined, in the early part of 1893, to adopt this system of lighting their trains, especially as, for economical reasons, the use of oil-gas on this line was not advisable. From the nature of the line in question one charging-station, situated at the Dortmund terminus, suffices for supplying the necessary current. The dynamo employed at night for the electric lighting of the station is used during the daytime for charging the batteries. The first-class coaches are provided with 16-candle-power glow-lamps, one for each compartment. In the second-class coaches the lamps are 10 candle-power. In the third-class there is a 10-candle-power lamp to each one or two compartments, according to the construction of the coach; whilst each fourth-class coach is furnished with two 6-candle-power lamps. At 15 volts these lamps are stated to require from 2 to 2·5 watts per candle-power.

The Author then fully describes the system of wiring, the arrangements for receiving the batteries under the car, and the appliances and indicator attached to each coach for switching off the lights and showing at any moment the capacity left in a battery. Where batteries are not quite discharged on arrival at the charging-station, but have not a sufficient capacity to run to the end of their journey and back for that particular coach, they are removed to one of an inferior class.

To obtain a potential of 15 volts the batteries consist of eight cells. They are of the same size for the different coaches, and only require charging once a day under the most unfavourable circumstances. The total capacity of a battery amounts to 90 ampere-hours, and the current required for all the lamps in a combination first- and second-class coach to 6·2 amperes, giving fourteen hours' light for each. The cells are of ebonite, each containing seven positive and eight negative plates, and are fitted in pairs into wooden boxes, which are covered with lead to protect them from the acid. Each cell is covered with vulcanized rubber, and the spaces between the cell and the sides and top of the box filled with an asphalt cement. From each cell a tube projects to allow of the escape of gases and the filling with acid.

A description is given of the charging-station and its equipment, together with the method of charging the batteries.

The cost of full equipment is shown to be as follows:—

	£	s.	£	s.
Buildings at charging-station			160	0
Equipment of same			560	0
Accumulators			462	10
Equipment of coaches—				
Six combination first- and second-class coaches with four glow-lamps at £27 10*s.*	165	0		
Twelve third-class coaches with three lamps at £22 10*s.*	270	0		
Eight fourth-class coaches with two lamps at £19	152	0		
One van with nine lamps	42	10		
Twenty-seven coaches containing eighty-five lamps	——		629	10
Apparatus for lighting charging-station			3	0
Total			1,815	0

The cost of adapting the existing coaches for electric lighting is included in the above. In the case of new ones these figures would be materially reduced. The saving in the cost of equipment of the coaches for the use of electricity instead of oil-gas, based on the higher figures, would amount to from £4 to £10 per coach. Allowing for renewals of lamps, gas for the motor, lubricants, wages, &c., and for depreciation of the accumulators at 6 per cent., the cost per lamp-hour averaging 10 standard candle-power amounts to 0·32*d.* Allowing for the necessary interest and sinking fund, this figure becomes 0·76*d.*

The following Table gives a comparison of the cost for different lines where a similar method of lighting is employed:—

——	Interest.	Sinking Fund.	Cost per Lamp-Hour, 10-Candle-Power Lamp.
	Per Cent.	Per Cent.	*d.*
Dortmund-Enscheder Railway	4	3	0·760
Jura-Simplon Railway	4	4–6	0·492
Danish State Railway	4	3–5	0·738
Chesapeake and Ohio Railway	6	6	0·228

The Author concludes by enumerating the disadvantages of lighting trains by means of oil-gas, and points out the superiority of electric lighting. He advocates the more extended use of electricity in place of gas, and draws attention to the experiments made by the Prussian State railways on the Berlin and Frankfort line, which, he states, proved very satisfactory. Before the larger companies, whose trains travel over the lines of other systems, proceed to introduce electric lighting, it would be advisable, the Author considers, for the German Railway Union to inquire into

the question and pronounce upon the most advantageous potential and type of battery, &c., so as to obtain a general uniformity of equipment.

Illustrations and diagrams accompany the article to show details of the plant.

J. R. B.

On the Electrical Discharge of the Torpedo (Torpedo vulgaris).

By — D'ARSONVAL.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxxi., 1895, p. 145.)

The Author describes some experiments by which the electrical discharge of the torpedo may be rendered visible. The fish is placed in a shallow tank of sea-water, on the metallic bottom of which it rests. A sheet of tinfoil laid on the dorsal surface of the electric organ forms one electrode, the other being the tank. The circuit is completed through three small 4-volt glow-lamps which may be either in series or in parallel. On exciting the discharge by pinching the fish, the lamps glow brilliantly for a fraction of a second, and may even be broken if the shock is strong.

By passing the discharge through a Ruhmkorff coil, the Author succeeded in making it illuminate a Geissler tube. He estimates the potential at from 8 volts to 17 volts, and the current at from 1 ampere to 7 amperes, but the duration of the discharge is only from $\frac{1}{30}$ to $\frac{1}{10}$ of a second.

G. J. B.

The Dey-Griswold Hydraulic Gearing for Electric Cars.

(The Electrical Engineer, New York, vol. xx., 1895, p. 130.)

The object in view in this apparatus is to enable a motor to run at a constant speed independently of the speed of the car. One motor only is employed and this is suspended by springs in the centre of the truck. The field-magnet has ten poles, and both it and the armature, which is drum-wound, revolve, being mounted on separate shafts. Four brushes bearing on the end of the commutator convey the current.

Each shaft carries a pump with four cylinders arranged radially. The pistons are single-acting and all are connected in the same plane to one crank-pin which can be moved $1\frac{1}{2}$ inch on either side of the centre while in motion. On the car-axles are placed "gear" type fluid motors connected to the pumps by flexible tubes; the pumps and motors being "in series" and alternating with one another. The fluid used is oil.

The crank-pin is moved by means of a sprocket-wheel actuating screw-gear, minutely described, and is locked in any position.

The motor running at constant speed, the fluid motors revolve with a speed proportional to the pumping capacity of the pumps, that is, to the eccentricity of the crank-pin, and when this is central their motion is nil; if it be carried past the centre the direction of rotation of the axles is reversed. If the speed of the car increases above the normal, the fluid motors drive the pumps which in turn cause the motor to run as a generator, thus effectually braking the car.

A very powerful starting torque can be obtained by giving only a very small eccentricity to the crank-pin, and starting is further aided by the momentum of the motor. In case of slipping, an advantage is gained by the two axles being practically mechanically connected through the intervention of the oil.

The more important advantages claimed are: saving in current owing to the absence of heavy starting currents; economy through the utilization of energy acquired by the car in descending gradients; reduction in weight by the use of one motor instead of two and by revolving both field and armature; ease of handling car and ability to stop quickly; and, since the motor runs at a constant speed, the system admits of the use of synchronous alternating motors.

C. H. W.

The Action of Electric Currents on Mine-Surveying Instruments.

By W. Lenz.

(Glückauf, 1895, p. 1197.)

In view of the rapid increase in the number of electric railways in the Westphalian coalfield and in the use of electric power under ground, the question of the action of electric currents on magnetic mine-surveying instruments is of such great interest that the Author has been induced to conduct a series of experiments. A point, under ground, was selected at a horizontal distance of some 100 yards from the rails of the Bochum-Herne Electric Railway, and 434 metres (1,420 feet) below it. There, by means of a Fennel's magnetometer with quartz fibre suspension, a series of observations of variation were made based on a fixed line. The magnetometer was previously compared for a long period with the apparatus in the Bochum town park, and the two instruments were found to coincide almost exactly. The first observation, in September, 1895, was made by day, the second by night, when the line was free from current, and the last again by day. Whilst the curve of the day-results exhibited great irregularities, that of the night-results was perfectly regular and in accord with the magnetic records. The irregularities in quite small intervals of time amounted to 2·7 minutes to 5·4 minutes. As at first, it was thought that the deviations might be ascribed to the iron-free safety-lamps employed. A

third observation was made in the morning, the lighting being effected by a stearine candle. The results were exactly the same as on the first day. As the observations were made at a comparatively large distance from other workings, and as the shaft was 200 yards away, it is evident that magnetic observations can, under such conditions, be only satisfactorily conducted during the night in the absence of the electric current.

Another source of error is the safety-lamp. Composed of various metals, the lamp in a hot condition sets up thermo-electric currents which act on the magnetic needle. In order to obtain information on this point, the Author placed six mine-surveyors' safety-lamps free from iron, one at a time, first in a cold condition then heated at the pole of a sensitive magnetometer. Of the six lamps examined, two, when cold, had no action on the needle, whilst all acted on it when hot. The deviations observed amounted to from 30 seconds to 160 seconds. A new benzene lamp, that had not previously been used, caused a deviation of as much as five minutes. The deviation increased with the temperature of the lamp. A quite new aluminium safety-lamp caused the same deviation when cold as when hot. From these results it follows that the mine-surveyor, before making magnetic observations with delicate instruments, should carefully test his lamp. The influence of slight magnetic properties may be lessened by holding the light in the prolongation of the magnetic axis. With side-lighting great care is necessary.

B. H. B.

Water-Power and Electric Transmission.

(The Engineering and Mining Journal, New York, 21st September, 1895.)

A power-station with electric transmission has recently been completed in Fitchburg, Mass., which has some features of special interest, and is attracting much attention. The water supply is obtained from the Wanoosnoo River, which has a minimum flow of 2,000 cubic feet per minute. This is brought to the power-house in a 36-inch pipe-line 1,800 feet long, terminating in a receiver 48-inch diameter, with which the wheels are connected by lateral branches.

The power-station is located about 3 miles from the city of Fitchburg, and consists of six Pelton wheels 28-inch diameter, having an aggregate capacity of 600 HP., all running on one shaft inclosed in three separate compartments. The wheels run under a head of 130 feet at 135 revolutions per minute, and are directly connected to a 300-kilowatt Westinghouse two-phase generator. The current is generated at a potential of 2,250 volts, and handled at this pressure on a switchboard, whence it is transferred directly to the transmission circuit. The exciter is run by a separate

Pelton wheel at 1,150 revolutions carrying 110 volts. The power thus generated is transmitted to the Simmonds Saw Factory, 2¼ miles distant, where the potential is reduced to 220 volts by means of two 100-kilowatt Westinghouse transformers, the current operating eleven motors of various capacities, these being of the Tesla self-starting type.

The speed of the wheels is controlled by a Pelton differential governor, and the regulation afforded is entirely satisfactory under what is regarded as the most difficult conditions anywhere to be found, owing to the constant and wide variations of load.

The entire power is used by the Simmonds Saw-Works, one of the largest factories of the kind in the country, and the working of the power-station has been so satisfactory that a material enlargement has already been decided upon.

Comparative Trials of the Work absorbed by Ropes and Belting.

By V. Dubreuil.

(Mémoires et Compte-rendu des travaux de la Société des Ingénieurs Civils de France, July, 1895, p. 28.)

The Author gives his experience of working belt- and rope-transmissions of motion, and submits several formulas to be used in the calculations for such installations. In comparing the two systems, the Author considers them from the several points of power absorbed, slip, first cost, suitability and working; and while he is of opinion that from the first of these points the two systems are fairly equal, from the rest he pronounces in favour of ropes. He is much impressed by the smooth and quiet working of the ropes, and their great sensitiveness to irregular loads, due to the elasticity of the separate strands. The Paper deals with the details of rope and belt transmission installations, more especially the former. The Author considering the relative sizes of pulleys and ropes, the distance between shafts, size of bearings and shafts, number and description of ropes, form of grooves, and supports for the bearings. He is in favour, contrary to usual practice, of having the tension side of the ropes above the pulleys, unless the distance between the pulleys is less than 23 feet to 26 feet, or the ratio of the diameters of the pulleys is less than 1 to 3. He points out also that the pulleys should be as light as possible and well balanced, so as to avoid stresses due to their inertia. For the form of groove he favours a V-shape, the sides being inclined at an angle varying from 38° to 48° according to the relative positions of the pulleys. Some space is devoted to the consideration of the supports for the bearings. The Author is in favour of metal frames resting on the walls, or on pillars suitably placed, the stresses between the pulleys being taken by the frames.

In relation to the question of the power absorbed, the Author gives at length the report of some experiments made in 1894 at Lille by a commission presided over by himself under the auspices of the North of France Industrial Society. These experiments were made with an engine having two fly-wheels, one provided with rope-grooves, and the other suitable for belt-driving; and a dynamo provided with two corresponding pulleys. The load, consisting of a number of glow-lamps, was kept constant, voltmeter and ammeter readings being taken every ten minutes, while the total number of revolutions of both engine and dynamo were recorded and indicator diagrams taken at similar intervals. Trials were made with hemp ropes, cotton belt, and two kinds of leather belt, each trial lasting two hours twenty minutes. The commission reports that the power absorbed by the two systems is almost the same, but the slip of the ropes is less than that of the belts. For the ropes, this slip amounted to 0·329 per cent., for the cotton belt, 0·780 per cent., and for the leather belts 0·780 per cent. and 0·961 per cent. respectively. The voltmeter reading is given but not the total watts, and the efficiency of the transmission cannot therefore be determined. The tables of readings are appended to the report.

The Author in concluding again draws attention to the capability of ropes to absorb variations of speed and power in either the driving or the driven shafts, and also to their smaller cost as compared with belting. A table of various installations of rope- and belt-transmission is also appended to the Paper, giving the details of horse-power transmitted, size and spacing of pulleys, sizes and speeds of ropes and belts, annual cost of maintenance, life, &c. Seven explanatory cuts are embodied in the text.

R. B. M.

Lighting by Luminescence. By A. Witz.

(L'Électricien, vol. x., p. 134.)

The Author points out that the present sources of artificial lighting consist of solids or liquids in an incandescent state. T ensure a white light and complete spectrum, a temperature of over 1,100° C. must be obtained. When analyzing the spectrum, the well known fact is found that a large proportion of the radiant energy is in the form of invisible heat-rays. The Author refers to a note by himself, showing the exact lighting efficiencies of such sources, published in the Comptes Rendus, 1891, vol. cvii. The search for methods for obtaining better efficiencies in artificial lighting has engrossed many scientific men, and some claim that the solution is to be sought in luminescent substances. The Author has attempted to obtain the true efficiencies of present means of

lighting by luminesence, and to compare the efficiencies with those obtained by incandescent means. He experimented with several bulbs containing rarefied gases, and specially mentions a miner's lamp and a small glow-lamp used for surgical purposes. The miner's lamp could be easily illuminated by a Ruhmskorf coil giving a 0·08-inch spark, and gave sufficient light to enable a table of logarithms to be read at a distance of 16 inches from the lamp. This is a rough gauge of the light, but the pale tint does not lend itself to photometric observations. Under these circumstances there was a current of 0·27 milliampere flowing at a potential difference of 4,190 volts. The Author, assuming that there is no lag, estimates the power at 1·13 watt, which is large considering the small light produced. The next results are obtained by measuring the power required to drive a Holtz machine at constant speed under the following conditions:—

The foot-pounds per second when driving the machine were as follows:—

On open circuit	7·5
„ short „	12·9
Sparks 5¾ inches long, eight per second	15·7
On the miner's lamp	12·4
„ surgical lamp	12·7

From this the Author argues that it takes 12·4 − 7·5 = 4·9 foot-pounds per second to light the miner's lamp = 6·6 watts. This, with the ordinary method of lighting, would yield at least two candle-power, whereas the light emitted by luminescence did not approach that figure. The Author extended his experiment by determining the heat emitted, and concludes that one-fifth of the energy of the lamp was in the form of heat-rays. Hence, although the external power applied is great, at present the method is in itself efficient, and with improved knowledge may become commercial.

R. W. W.

The Sabin-Hampton "Express" Telephone Switchboard System.

(The Electrical Engineer, New York, vol. xx., 1895, p. 173.)

It is pointed out that the multiple system of switching causes operating expenses per subscriber to increase as the number of subscribers is augmented, and in the system described it is sought to avoid this, and to prevent the chance interruption of connection which is liable to occur through careless testing of engaged lines.

In the "Express" system, there is no repetition of the line on several boards, and hence there is no danger of a line already in use being plugged on to another subscriber. The work of making a connection is divided among three operators, known respectively as the receiving, intermediate and calling operator.

On a subscriber removing his receiver from its hook, a battery

circuit is completed automatically with the result that an indicator drops at the exchange. The receiving operator then inserts a plug in the subscriber's jack, breaking the circuit through the indicator, which returns to its normal position, and connecting the subscriber to a line on the intermediate operator's board. To this line are connected two relays, each of which causes a lamp to light when excited. On seeing the lamp light up, the operator depresses a key and speaks to the subscriber, afterwards asking the calling operator, who controls the required number, to connect it to an interconnecting wire which is specified by the latter, who rings up the subscribers wanted.

So long as the calling subscriber's receiver is off the hook, the first lamp is lighted, and the second lamp referred to is also alight until the listening key is depressed, relighting if no connection is made. As soon as the calling subscriber hangs up his receiver, the lamps go out, but a third lamp is lighted, the relay completing its circuit on being demagnetised. A fourth lamp, controlled by an interlocking relay is lighted when the operator rings up the called subscriber and goes out on his replying. When the receivers are hung up at the close of the conversation, this relay drops a shutter, thus notifying to the calling operator that subscribers have finished speaking.

In practice, the same operator attends to both receiving and calling, a different operator looking after the intermediate boards.

The system is in use in San Francisco with satisfactory results.

C. H. W.

Properties of Fuse-Metals when subjected to Short Circuits.

By Walter E. Harrington.

(Transactions of the American Institute of Electrical Engineers, 1895, p. 332.)

The Author refers to the departure of fuse-wires from Preece's law ($C = a\,d^{\frac{3}{2}}$) observed with thick wires and when short circuits occur through them. In the absence of data giving the current that would flow through a given fuse-wire when a 500-volt generator was short-circuited through it, the Author carried out a series of experiments bearing on the question in a New Jersey power-station. The apparatus comprised a special fuse block, having its terminals protected by oil, 3 inches of the fuse wire under test being exposed to the atmosphere, in series with which was placed a carefully calibrated magnetic circuit-breaker, a 100-ampere knife-blade switch being used to complete the circuit. Current was obtained from the station omnibus bars, 30 feet away, being conveyed by two No. 2 B. & S. gauge copper wires.

The fuse-metal being in position, the switch was closed and the wire fused; the experiment was repeated until the adjustment of

the circuit-breaker, which would just cause it not to open, was determined, and also that which would cause it to just open. The current actually flowing under the short circuit was assumed to be between the two values of the steady current which would actuate the magnetic cut-out in the same manner as determined by previous calibration. At the time the tests were made, the station was lightly loaded, and a 200-kilowatt four-pole and a 100-kilowatt two-pole machine were running.

The Author experimented with wires of copper, aluminium, and an alloy of lead and tin, the pressure maintained between the short-circuited terminals being 500 volts, and the mean of the two currents referred to above being taken as the actual current flowing.

The results are tabulated, and the Author considers that the law determining the current passing when the short circuit takes place is represented with fair approximation by the formula $C = B d^2$, where C is the current in amperes passing through the fuse-metal, d is the diameter of the wire in inches, and B a constant depending on the metal employed and on the voltage between the terminals short-circuited. Under the conditions named, the average value of B was found to be 460,000 for copper, 392,000 for aluminium, and 118,000 for the lead-tin alloy. The actual values of the "constants" vary as much as 28 per cent. from the mean in the case of copper, 27 per cent. for aluminium, and 32 per cent. for the alloy. It was observed that the current flowing was closely proportional to the area of the wire, the current being obtained by dividing the area expressed in circular mils by 1·9 in the case of copper, by 2·6 for aluminium, and by 9 for the alloy.

As regards the behaviour of the several materials when fusing, copper gave a loud explosive report with but little flash; with aluminium the metal seemed to burn for a longer time, and pieces of burning metal dropped down after the explosion, while the lead-tin alloy gave off heavy suffocating fumes.

A Table is given showing the area in circular mils of wires from No. 4 to No. 30 B. & S. gauge, and the fusing currents for copper as calculated from Preece's formula, and also when a 500-volt short-circuit occurs through the wire as calculated from the law enunciated by the Author; as an indication of the correctness of the latter, it is stated that values obtained by extrapolation beyond the limits of the actual experiments fulfilled the requirements.

C. H. W.

On a New Method of Measuring Temperature.

By Daniel Berthelot.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxx., 1895, p. 831.)

The method of measuring high temperatures by the air thermometer is complicated by the necessity of allowing for possible changes in the volume of the containing vessel. The Author overcomes this difficulty by calculating the temperature from the observed change in the refractive index of a gas. A ray of light is divided, by an interference apparatus, into two parts, one of which traverses the tube to be heated, and the other passes through a similar tube, containing the same gas, for comparison. The pressure and temperature of both being equal, the position of the interference bands is noted. The experimental tube is now raised to the temperature which it is proposed to determine, causing a displacement of the fringes, which are brought back to their original position by reducing the pressure in the comparison tube. The calculation is made on the assumption that a gas has the same index of refraction for a given density, although the temperature and pressure may be different. The disturbing effect of the region of varying temperature at the ends of the experimental tube is eliminated by making two sets of observations with tubes of different lengths.

The experiments quoted relate only to temperatures between 0° C. and 200° C., but the Author is investigating the applicability of the method to the study of the electric furnace.

G. J. B.

On the Electrical Conductivity of certain Compounds near their Critical Temperatures. By Prof. A. Bartoli.

(L'Elettricità, July 7, 1895, p. 428.)

Continuing his previous researches,[1] the Author now finds that sulphur dioxide, which, in common with all the other anhydrides examined by him, possesses in the liquid state a feeble, but well marked conductivity, loses it entirely when raised above its critical temperature, 157° C., thus confirming the results obtained with methyl-alcohol in 1886. The substances experimented upon were enclosed in tubes of specially hard glass, 200 millimetres long and 5 millimetres internal diameter, furnished with electrodes of very fine platinum wire. To eliminate the effect of the possible conductivity of the glass at the high temperatures employed, a second similar tube, filled with dry air, was immersed in the same bath of petroleum during each experiment, and subjected to the same tests.

G. J. B.

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[1] Nuovo Cimento, vol. xx., 1886, p. 136.

On a Phenomenon of Phosphorescence in Rarefied Nitrogen.

By Gaston Segny.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxxi., 1895, p. 198.)

The Author finds that under certain circumstances nitrogen, like oxygen, is rendered phosphorescent by the electric discharge. The gas is placed in a vacuum tube formed of three large bulbs joined end to end, and furnished with electrodes, a small quantity of the vapour of tin di-chloride being introduced, together with the nitrogen. During the passage of the induction current, the tube glows with a rose-coloured light, and after its cessation a milky white phosphorescence is seen, which gradually dies away. The duration of this phenomenon is from ten seconds to one minute.

G. J. B.

On the Density of Helium. By — Clève.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxx., 1895, p. 1212.)

The Author states in a letter to Mr. Berthelot that Langlet, who has been working on the subject in the laboratory at Upsala, finds the density of helium to be 2·2 (hydrogen = 1). This result is considerably lower than that announced by Professor Ramsay. The gas was extracted from cleveite, and did not contain argon. It was freed from hydrogen by copper oxide, and from nitrogen by metallic magnesium.

G. J. B.

A New Compound of Argon. By Daniel Berthelot.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxx., 1895, p. 1316.)

A quantity of argon, in contact with liquid carbon disulphide, was submitted to the action of induction currents in an apparatus similar to that used for making ozone. After three hours 11 per cent. of the gas was absorbed, and by continuing the experiment it was found possible to cause not less than 56 per cent. of the argon to combine with the liquid. An accident put an end to the operation at this point. There was no visible fluorescence as in the case of the Author's experiments with argon and benzene. From the liquid so obtained the Author was able to regenerate argon, recognizing the gas as well by its spectrum as by its property, discovered by him, of forming a fluorescent compound with benzene.

G. J. B.

On the Action of Fluorine on Argon. By HENRI MOISSAN.

(Comptes Rendus de l'Académie des Sciences, Paris, vol. cxx., 1895, p. 966.)

Having received a sample of argon prepared by Professor Ramsay, the Author heated it with titanium, boron, and lithium, but found that these substances have no tendency to combine with it. He next attempted to produce a compound of argon and fluorine, by leading the two gases into a platinum tube 10 centimetres long and 2 centimetres in diameter, furnished with disks of fluor-spar at each end so that the course of the experiment might be observed. There was no visible reaction and no evolution of heat, and no change was produced by the passage of an induction discharge through the mixed gases.

G. J. B.

Calculating-Machines. By LÉON BOLLÉE.

(Bulletin de la Société d'Encouragement pour l'Industrie Nationale, 1895, p. 977.)

The Author presented to the society a description of a large calculating machine of a new type designed to perform mechanically the most complex arithmetical operations, and descriptions of a series of smaller machines based on known principles, to which he had added considerable improvements in details. The Committee of Arts examined these machines and reported to the society.

The first of these machines forms a flat metallic table, is based on the same principle as "Napier's bones," and is intended to facilitate operations of multiplication and division. The apparatus gives the partial products occurring in these calculations, while the additions are performed by the operator.

The second apparatus performs similar functions, but is presented in a more compact form. It consists of six cylinders, each carrying on its circumference the series of numbers inscribed on "Napier's bones." The topmost numbers on the cylinders can be read through slits in the top of the frame. By means of a sliding shutter it is easy to read off the partial products occurring in the multiplication of two numbers; the operator adds the partial products, and so completes the calculation. This machine, carrying six cylinders, and giving consequently products up to thirteen figures, measures only 6 inches by 2½ inches, and can be made for a small sum.

The third apparatus is designed to make arithmetical calculations entirely mechanical, and is less costly than an ordinary arithmometer.

The fourth apparatus, the great calculating machine, not only performs multiplications and divisions mechanically, but also the extraction of roots. One turn of the handle of this machine

gives the partial products in multiplication; while in the ordinary arithmometers as many turns have to be made with the handle as is represented by the multiplier figure. A system of interlocking is so arranged that the machine absolutely refuses to perform an impossible or false calculation.

The report, which is signed by General Sébert, is accompanied by descriptive notices (illustrated by eight woodcuts) by the inventor.

A. S.

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MINUTES OF PROCEEDINGS,

1895-96.—Part I.

N.B.—Titles in italics refer to Original Papers, and those selected for printing only are further distinguished by the suffix "(S.)." Abstracted Papers are not so indicated.

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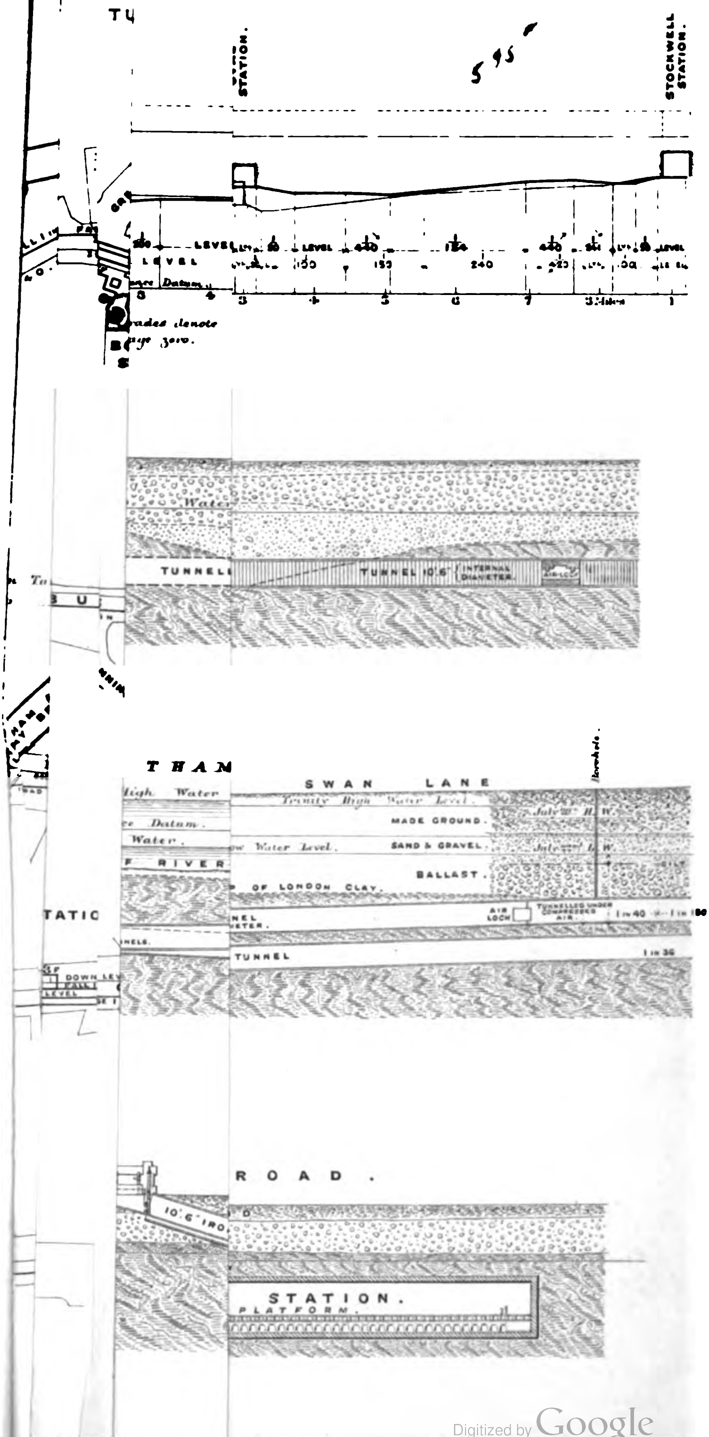
PLATE 1.



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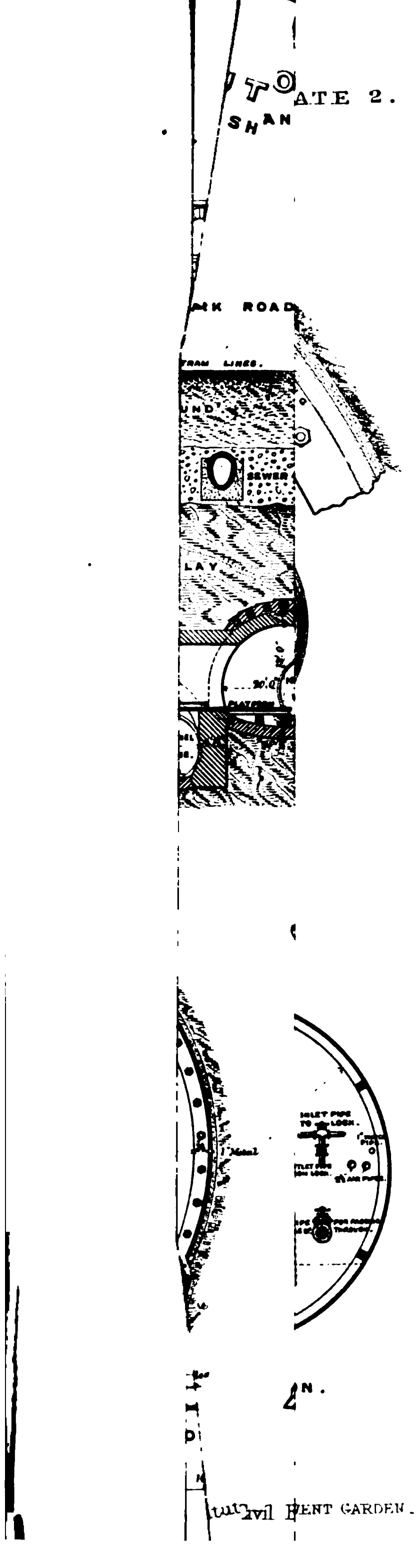
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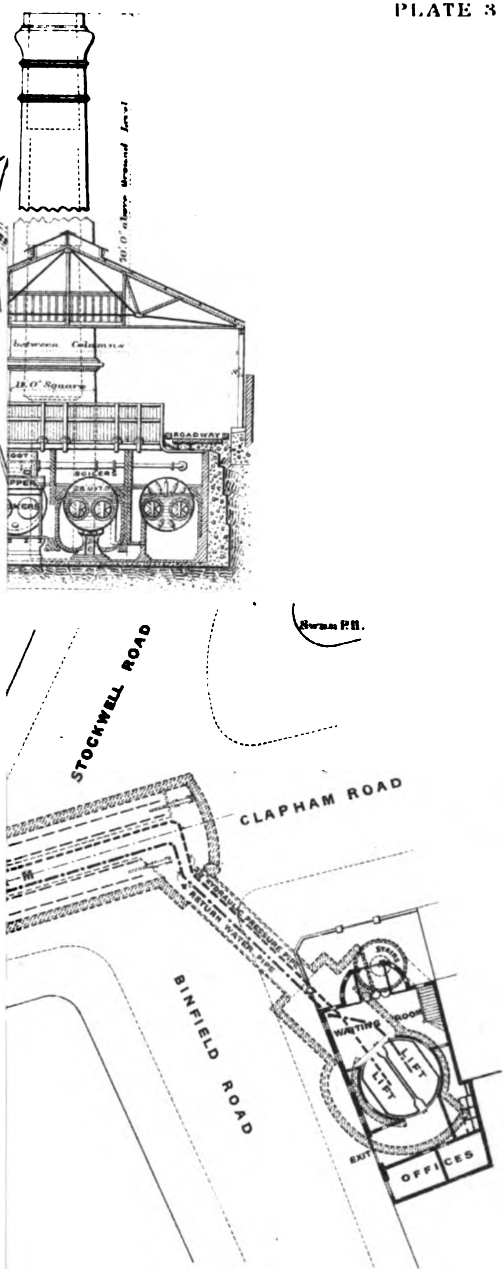

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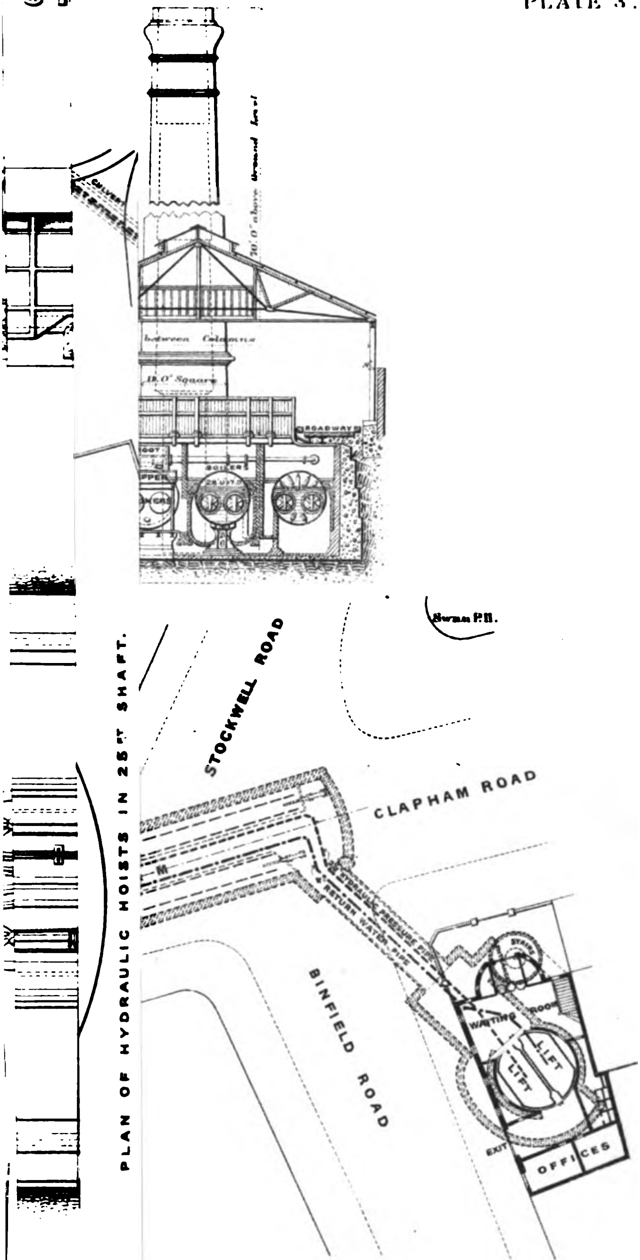

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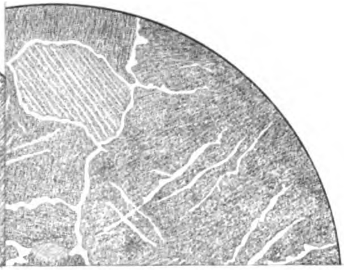

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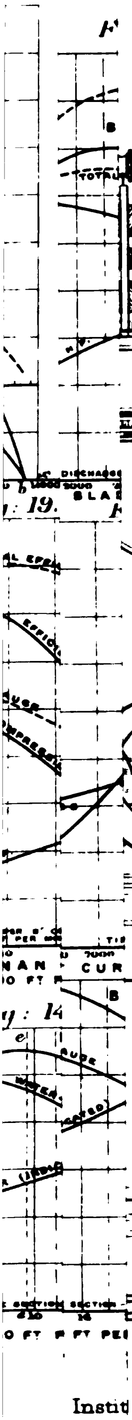

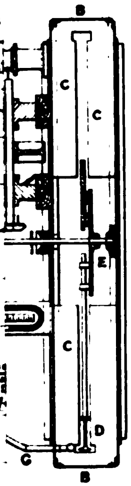

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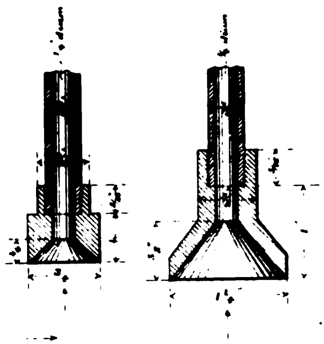

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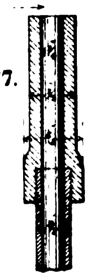

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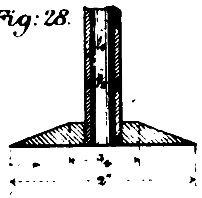

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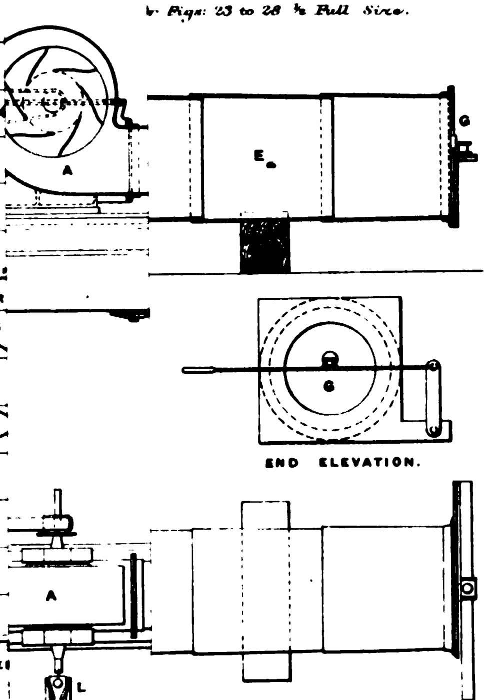

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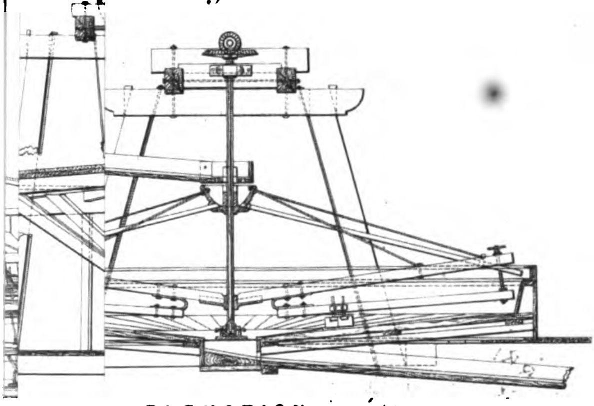

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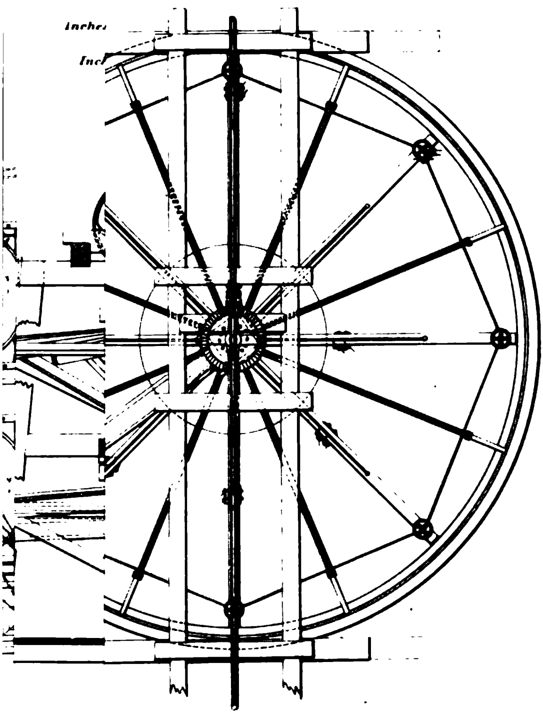

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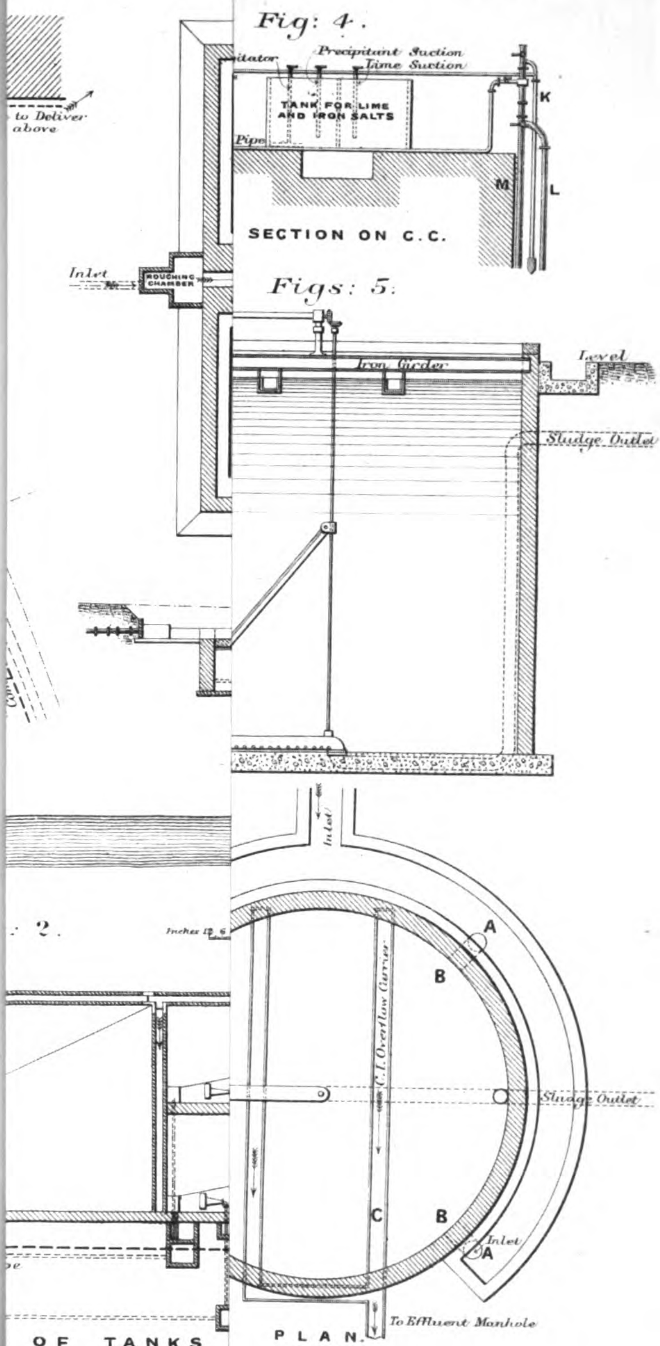

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